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Aristotle and Theories of Justice*

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Today it is all the rage for political theorists and even philosophers to have theories of justice. Looking back on the history of political thought, we cannot help but notice that not all previous philosophers have taken justice and theories of justice so seriously. Among those who did not was Aristotle. To be sure, he had a theory of justice, and from this fact we might infer that he thought it necessary to have one. But I shall presently argue, primarily from Aristotle's treatment of the problem in the Nicomachean Ethics, *that Aristotle thought all theories of justice, including his own, to be unsatisfactory. In his opinion, a politics that understands its highest purpose as justice and a political science that attempts to comprehend all political phenomena within a theory of justice are practically and theoretically unsound.*

Today it is all the rage for political theorists and even philosophers to have and to expound theories of justice.[1] Looking back on the history of political thought, we cannot help noticing that not all philosophers have taken justice and theories of justice so seriously. Among those who did not was Aristotle. To be sure, he had a theory of justice, and from this fact we might infer that he thought it necessary to have one. But the argument that I shall make is that Aristotle thought all theories of justice, including his own, to be insufficient.[2] In his opinion, a politics that understands its highest purpose as justice and a political science that attempts to comprehend all political phenomena within a theory of justice are practically and theoretically unsound.

Aristotle's theory of justice is perhaps best understood by understanding its place in the *Nicomachean Ethics.*[3] The *Ethics* as a whole is meant to be a comprehensive investigation of the end of human action, which is the human good, or happiness (1094a 1–3, 1094a 18–26, 1097b 20–21, 1179a 33–b 4). Since the science which has this study as its object is political science, the *Ethics* is "a sort of political science" (1094a 24–29, 1094b 10–11). The premise of the *Ethics* is that the core of happiness is the practice of virtue (1098a 16–18). Virtue is divided into virtue of character, or moral virtue, (*ethike*) and intellectual virtue (*dianoetike*) (1103a 3–7). Or sometimes Aristotle says that the subject matter of political science is the noble and the just (1094b 14–16). According to this formulation, what we call moral virtue is treated in terms of what seem to be its principal components, nobility and justice. In the discussion of the particular moral virtues in Books 3–5, it becomes clear that both the pride, or greatness of soul, characteristic of nobility and justice are comprehensive virtues. That the noble and the just are not components of a unified morality, that they might at times be incompatible, or that they point to and reflect opposed principles of morality is not suggested in Book 1, because the working hypothesis is that the good of the city, and therefore the just, and the perfection of the individual, and therefore the noble, are roughly the same and effected by the same means.[4]

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[1] The rage reached epidemic proportions after the publication of Rawls (1971) and Nozick (1974). A comprehensive bibliography of the literature spawned by Rawls' *A Theory of Justice* can be found in *Political Theory* (Nov., 1977).

[2] As will become obvious, in presenting my argument I have not made frequent reference to the secondary literature on Aristotle. My argument is uncommon not so much because it is opposed in the literature, but because analyses of Aristotle's treatment of justice as a whole are generally lacking. Many of the commentaries are very helpful in clarifying details. But for the most part, they fail to do the two things I have attempted to do here: to suggest the possible implications of Aristotle's explicit statements and to treat them as if they were components of a coherent, if dialectical, argument with a point and a purpose. My attempt has led me to state a somewhat surprising and offensive conclusion, at which the very decent commentators might have balked in any case. The standard modern works on the *Ethics* or on Book 5 in particular are Burnet (1900), Gauthier-Jolif (1958–59), Grant (1885), Hamburger (1965), Hardie (1968), Jackson (1973), Joachim (1951), Ritchie (1894), Ross (1923), Stewart (1973), Thomas Aquinas (1964), Vinogradoff (1922). Notable works not subject to the above criticism are Faulkner (1972), Jaffa (1952), and Ritchie (1894). Cropsey's (1977) fine essay on justice and friendship came to my attention after this article was completed. Gauthier-Jolif (1958–59), and to a lesser extent Hardie (1968), are also useful.

[3] In my references to Aristotle's works, all Bekker numbers cited, unless otherwise specified, refer to the *Nicomachean Ethics.*

Justice is also treated in the *Eudemian Ethics* and the *Magna Moralia* as well as in the *Politics,* but in the *Nicomachean Ethics* it is treated thematically and at length. More important for present purposes, the structure of the *Ethics* as a whole, and therefore of the place of the theory of justice in Aristotle's moral and political philosophy as a whole, is perhaps easiest to grasp. In making my argument, I necessarily assume that the *Nicomachean Ethics* was written by Aristotle and that the text we have is at least roughly in the form intended by the author. Speculations about the authorship of the *Nicomachean Ethics* and its integrity are reviewed, for example, in Grant (1885, Vol. 1, pp. 1–171), Hamburger (1965, pp. 1–6), and Jackson (1973, pp. xxii–xxxii).

In any case, I am fairly confident that nothing in the *Politics* or elsewhere is fundamentally inconsistent with the teaching of the *Nicomachean Ethics.* Consider, for example, *Politics,* 1323a 27–34. For reasons that will become clear later, it is significant to note that in the *Politics,* too, Aristotle conceives of friendship as an improvement upon justice and identifies aristocracy most closely with friendship. He rarely, if ever, speaks of the justice, as distinguished from the goodness of aristocracy. For the superiority of friendship to justice generally, cf. 1262b 7–8, 1263b 29–37, 1287b 30–35, 1295b 23–24.

Justice is the last of the virtues of character which Aristotle treats, and its treatment is followed by that of the intellectual virtues. This placement reflects the fact that it forms a bridge of sorts between them, not only because justice is shown to require discriminating judgment as well as good character, but because the analysis reveals that the ground of the moral virtues is problematic. Justice is the only virtue to which an entire book of the *Ethics* is devoted, as if to emphasize its importance. Only to friendship, which is the unitary subject of Books 8 and 9, is a larger solid block of argumentation devoted. Because justice and friendship are said to be concerned with the same things (1115a 22–24, 1159b 25–26), we must consider at some length friendship as well as justice.

The demand for a theory of justice arises from political practice (1129a 6–10). The theory formulated to meet that demand may seem more or less adequate in practice, but as I shall argue, it is not adequate to satisfy a demand for knowledge of politics. Consequently, if it is adequate in practice, it must be so for the wrong reasons. In what follows, I shall comment on how the theory of justice emerges in Book 5 and what that theory is. I shall then show how in Book 5 Aristotle raises objections to his own theory, or at least forces his reader to raise them, and how he indicates that these objections can be met in the context of a teaching on friendship, not justice. To anticipate, Aristotle's central objections to the theory of justice are of two sorts: First, it seems that a theory, like any art or science, must embody knowledge of universals to merit the name science, but the universals of politics are not true universals in the sense that they fit *all* cases, and in politics the particulars, especially the particular exceptions to the general rule, may be worthy of more serious consideration than the universals. Second, if justice as the practice of virtue toward others requires a disregarding of virtues conducive to one's own good, to insist upon this disregard may be not only to ask the impossible of human beings, but to ask the undesirable as well. Consequently, we could not consistently defend as correct and beneficial a political science which is nothing more than a prescription for justice.

Aristotle begins his inquiry into justice as he begins all such inquiries, with what is first for us (1095a 30–b 4, 1129a 5–6), hence with the kind of questions someone serious about morality and politics would ask. What we commonly mean by justice is that it is a habit of some sort which issues in actions that we could call just and which assures that these actions are undertaken with the intention of their being just (1129a 6–10). The minimal demands for a theory of justice adequate to common opinions are, then, that it enable us, first, to distinguish just from unjust actions and, second, to establish a connection between consequences and intentions. To meet these demands we would need a definition of the just and a plausible explanation of human behavior.

As Aristotle remarks (1129b 17–23), as we might know good condition of body and its cause from knowing bodies in good condition, so we must begin by assuming that we have an adequate perception of just actions, from which we can make inferences about the just and justice. More precisely, both his contemporaries and we now are prone to attribute injustice to those who act outside the law and to those who take more than their fair share.[5] Now if we call what is illegal unjust, must we not be supposing that what is legal is just? And similarly, if we say that in taking too much, someone is unjust, must we not be supposing that it is desirable and possible to determine the right amount, or the "equal?" The just, then, appears to have two meanings, the legal and the equal (1129a 32–34).[6]

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[4] 1094b 7–10. This assumption is questioned at 1130b 26–29 and at 1180b 23–25.

[5] To identify the just and the legal may strike the contemporary reader as passé, if not incorrect, but first of all, Aristotle speaks here in the name of

In attempting to understand why Aristotle reports the opinion that the legal is or intends to be the just, it is perhaps useful for us to think not of laws like the hedges of Hobbes and Locke (Hobbes, 1968, p. 388; Locke, 1960, p. 348), but of religious and customary laws, written and unwritten, which correspond to Aristotle's dictum, so strange to ears of liberals, that what the laws do not command, they forbid (1138a 7). For Aristotle, the legitimacy of law and the propriety of unreflective obedience to laws are grounded in the presumption that the intention of law is to secure the happiness of the political community and its parts and that the law of a particular community fulfills the intention more or less well. At the same time, since happiness is said to be a consequence of the practice of virtue, we can say that law intends to prescribe the practice of virtue, or of all the virtues. If the legal is the just, then justice as law-abidingness must be the whole of virtue. But more precisely, justice is said to be the practice of complete virtue *toward others,* and from this we might infer that justice is a concern for the good of others. Aristotle concludes this elaboration with the comment that justice as law-abidingness is the whole of virtue, but that it differs essentially from virtue. Only at the end of Book 5 does the reader fully understand this, but we might remark now that virtue, having been introduced as the condition of happiness, reflects a primary concern for one's own good, as distinguished from that of others. No wonder the proverbial wisdom finds justice more amazing than the evening or the morning star (1129b 11–1130a 13).

Aristotle considers first not justice as virtue as a whole, or law-abidingness, but the justice which he insists is a part of virtue (as courage is a part of virtue). This partial justice is justice in the sense of taking one's fair or equal share of good and bad things. Upon reflection, we can see how the first consideration presupposes this examination. The presumption that law secures the happiness of the political community and its parts is ultimately a presumption that the law was made by some one or many who thought about what is good for the whole and its parts and distributed whatever good the political community can supply in accordance with this determination. Our respect for the law leads us to suppose that some legislator has employed the principle of partial justice on behalf of future citizens; our suspicion that it may not have been employed properly—that we live in an imperfectly just regime—leads us to presume to apply the principle ourselves. Partial justice should give us a standard for law, although to think about this is potentially subversive of law. In the discussion of partial justice that follows, Aristotle makes us assume the perspective of a legislator, or at least of a judge. But we, as distinguished from the hypothetical legislator, already live in a regime toward whose probable injustice we are likely to have some animus. For this reason, our judgments might reflect partisan passions and even anger. We might be tempted to criticize from the perspective of what is most conducive to our own benefit. In what follows, Aristotle presents what is for him an unusually abstract account of partial justice, perhaps to show us what the perspective of an impartial legislator would have to be. I believe that he does so primarily to teach us the habit of justice: One who wishes to be just must first learn to calm

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common opinion, and he himself later questions the identification. Even we ought not to disregard the opinion of our own usually "silent majority" on this issue. Furthermore, many of those who purport to deny that the legal is the just do admit that disobedience is the exception rather than the rule, while seeking a principle to justify their deviation. And, in fact, since that justification often assumes the form of an appeal to a "higher" law or universal principles of conscience or reason, they concede that the attempt to identify the just with some legal order is not unreasonable. While less willing to attack the law publicly, Aristotle is less convinced than many of us that there are universal principles of justice in the name of which to attack laws that appear to deviate from the just.

[6] As Gauthier and Jolif (1958–59) point out, Aristotle's method of inquiry is not merely to begin from common opinions, but to make language as precise as possible (Vol. 2, Pt. 1, p. 330). They correctly note that Aristotle belabors the distinction between the meanings of the just as the legal and the equal, as if to say that in common opinion the two were used equivocally and not sufficiently distinguished (p. 336). This common tendency to identify the legal and the equal they would trace to the democratic context in which Aristotle wrote (pp. 325–27), although Aristotle himself does not do so. Rather he suggests that all law necessarily tends to be egalitarian. Consequently, his later insistence on the formulation of the just as equal shares to equal *and* unequal to *unequals* serves to reveal an inevitable tension in the notion of justice.

the anger which is at the core of righteous indignation at injustice and to overcome the natural, unreflective, concern for one's own good.

The theory of justice Aristotle presents is surprisingly simple. Partial justice is divided into two forms: distributive and corrective (1130b 30–1131a 1). Distributive justice provides the principle underlying the distribution of goods and honors in a political community; it is the principle embodied in a regime. The general principle, that equal persons must have equal shares and unequals, unequal shares, can be stated with the certitude, clarity, and precision of a mathematical formula. Distributive justice is a proportion. Corrective justice provides the principle applied in courts of law when contracts must be rectified. Here persons are not to be taken into account, but the gain reaped from inflicting loss on a partner in contract is to be equalized by a judge who, again with impressive mathematical rigor, imposes a fitting loss on the one who has gained unjustly. At the conclusion of this elaboration, Aristotle remarks that mathematical appearance notwithstanding, neither form of justice is simple reciprocity or retaliation (1132b 20). Whether this deviation from strict reciprocity should be seen as an improvement or a defect is never clearly stated.

Let us consider some peculiar details of the argument thus far. First of all, the theory of justice is emphatically mathematical in formulation, as if to suggest that such theories are properly mathematical. Since we speak of someone who is unjust as getting more or less than a fair share, we must necessarily be able to speak of an equal share (1131a 10–13); in our accusation of injustice we presuppose the kind of precise calculation of equality which is characteristic of mathematics. In the discussion of corrective justice Aristotle goes so far as to contend that the word for the judge who performs the mathematical calculation of gain and loss is etymologically derived from the word for "to halve" (1132a 32). What is most curious about this presentation is that Aristotle warns us on several occasions that we ought not to expect mathematical rigor from the political scientist because the subject matter of political science does not lend itself to such treatment (1094b 19–27, 1098a 26–33, 1165a 12–14).

Indeed, there are some difficulties in the theory. Few would deny that equal persons ought to have equal shares in good and bad, but as Aristotle notes in passing, people often disagree about what constitutes equality in persons (1131a 25–29). Are human beings to be deemed equal and deserving of equal shares because they are equally capable of contributing wealth or performing virtuous acts for the common good; because they are equally born of the human race; or because they are equally needy? Different answers to this question are the causes of regimes, which are for Aristotle the most important political phenomena (*Politics,* 1282b 10–13, 1289a 13–15, 1337a 11–14). A regime is "the regulation of offices in a city, with respect to the way in which they are distributed, what is sovereign in the regime, and what the end of each community is" (*Politics,* 1289a 15–18). Simplicity and rigor notwithstanding, according to the manifest teaching of the *Ethics* and the *Politics,* no significant political controversy would be resolved by the application of the principle of distributive justice as stated. It is too general, and it presupposes a prior resolution of the hardest political problem. Aristotle's rhetorical display obscures the controversial nature of the regime and, therefore, of justice.

The discussion of corrective justice, which oversees contractual relations, is also peculiar. What are classified as involuntary (as distinguished from voluntary) contracts are *crimes,* and, therefore, the corrections applied ought to include punishments. According to the formula as stated, nothing extra is to be taken from the criminal to satisfy the demand, born of anger, for punitive damages (1132a 24–1132b 11). By subsuming the whole problem of crime and punishment under an overtly economic terminology (1132a 10–12, 1132b 11–13), Aristotle undoubtedly means to make us forget about the potential violence of politics and the punitive consequences that inevitably accompany corrective judgments. Both one who might refrain from judging to avoid inflicting harm and another who might take particular pleasure in inflicting it would benefit from the oversight. Indeed, that political problems are solved neither by blatant vindictiveness nor by quasi-economic calculation is suggested by one of Aristotle's own examples (1132a 6–10). The judge must rectify the murderer's gain and the victim's loss; but how the dead victim's loss is rectified by the murderer's fitting loss of life is not obvious. Furthermore, whatever the transaction, the judge is required not to consider the persons involved (1132a 1–6). That judges ought not to discriminate in favor of friends in applying the law strikes us as reasonable, but we might as easily recall the observation that it is also illegal for the rich to sleep under bridges in Paris. The judge is permitted no more mercy than revenge or favoritism. The final difficulty we find in this corrective justice is that we must suppose that the initial distribution of goods, here restored, was fair (1132b 13–20). Corrective justice, however fair, is not shown to lead to any improvements, except insofar as it might be better to allow the taking of an eye for an eye instead of an eye, ear, nose and throat. If anything, the discussion of corrective justice serves to emphasize the importance of the problems of distributive justice and tempts us to think about the possibility and desirability of redistribution. If our objections to the theory of justice as stated are valid, then we really demand of the just not only rigor to prevent partiality, but flexibility to promote benefits. In sum, what Aristotle's presentation does thus far is to remind us that we settle for rigor in order to ensure that flexibility never becomes a weapon of the vindictive or tyrannical.

To posit as the principles of justice equal shares to equals and impartial legal correction is to conceive of justice as something like reciprocity (*antipeponthos*). But at times even to demand "an eye for an eye" might strike us as unjust. As Aristotle reminds us, it might not be fitting that a citizen strike back at an official and we might want to distinguish between voluntary and involuntary crimes. To conceive of justice as reciprocity, therefore, is to deny the relevance of a possible rank order of human beings and to forget that human beings are thought sometimes to be capable of deliberate action and sometimes not. In making an argument against this conception of what is just, Aristotle notes that the Pythagoreans and those who speak of Rhadamanthys did so conceive of it (1132b 21–28). We might surmise that the Pythagoreans did so because they found no theoretical difficulty in relating beings to one another on the basis of some assigned or discovered numerical value of each (*Metaphysics*, 985b 23, 986b 3). There would be a theoretical difficulty if the natures of beings were not quantifiable or commensurable and if it were necessary to ask whether and how they might constitute a rank order of being. We might surmise that those who spoke of Rhadamanthys—that is, all who believed in the Olympic gods—could so speak of justice because they imagined that human affairs were judged and all injustices eventually rectified by a divine being whose ability to read souls and whose fairness need not be doubted. If there were no such being to rely on, one would have to understand how justice might emanate from fallible human judgments. To argue that justice is reciprocity is thus to suppose that natures are not problematic or that nature as a whole, in its perfection, supports our efforts to secure justice. In defense of his contention that justice is not reciprocity, Aristotle grants that something like reciprocity, or perhaps something like the belief that justice is reciprocity, is essential to the maintenance of political communities (1132b 31–34). He denies that this is "the just simply," presumably because the justice necessary to political communities is in truth not grounded in a nature or a divinity as conceived of by either mathematical physicist or the pious.[7]

Rather, Aristotle proposes that the reciprocity of political communities originates in the willful demand to requite evil with evil (1132b 34–1133a 1), as well as in the inability of individuals to provide for all of their own needs (1133a 16–19). He thus acknowledges nature's imperfection, or at least the temporal priority of injustice, although he characteristically obscures the harshness of nature and human nature by emphasizing economics, speaking as if the exchange necessary for survival were an exchange not of harms, but of goods like beds and shoes.[8] Exchange is possible only if various goods are lacked and desired by some and supplied by others and only if the various goods can be made commensurate in value. While raising the question of whether things have value in themselves,[9] Aristotle explicitly says

[7] As Ritchie (1894, p. 190) succinctly puts it: "Particular Justice in both its forms has been explained in terms of mathematical formulae. [To use mathematical conceptions in ethics was for the Greeks to make ethics 'scientific,' to take the subject out of the level of mere popular moralizing by using the conceptions of the only science which by that time had made conspicuous progress and so come to be the type of scientific thought.] But it was the Pythagoreans who first introduced these mathematical formulae into ethics. They, however, defined Justice simply as 'Reciprocity.' " None of the commentators dwells on the significance of the mention of those who speak of Rhadamanthys and the error of either the believers or the god about the meaning of justice. The failure of Gauthier and Jolif (1958–59) to point this out is somewhat surprising, for they begin their discussion of Book 5 with the observation that Aristotle's silence about the traditional connection of justice and religion is remarkable (p. 325).

[8] Similarly, at 1133a 3–5 Aristotle takes as a sign that the city's justice is reciprocity temples to the Graces, which remind us of the need to exchange favors. He says nothing of shrines to *Dike*, which might remind us that justice entails reciprocal harms.

[9] Consider 1133b 18–28. To argue that the values of beds and houses are made commensurable by need or money is not to insist that these things have no intrinsic worth, but only that we cannot practically *compare* their worth.

that the true measure of value is human need (1133a 26–27). But human need, he contends, is too unstable to provide the security required to exchange with confidence. Money, the conventional character of which Aristotle emphasizes, must replace need as a more stable standard to guarantee exchange and, thereby, association (1133a 27–31, 1133b 10–23). We infer that convention or law generally is also necessary to lend stability and clarity to the definition of the relations of human beings to others and to things. If conventions serve this necessary purpose of minimizing inconveniences caused by the variation of nature and human nature, we must nevertheless see that they obscure that variation. The just established by means of laws and conventions is necessary, but it is apparently not natural.

While Aristotle speaks of the "nature" of justice, he speaks only of the "what" of the just and the unjust (1133b 29, 1134a 14–16). This difference reflects the problem his text has thus far led us to formulate. What is just seems to exist not by nature, but by convention. Convention or law, which properly fits the usual or the average case, assumes the form of a constant rule, serving as each community's statement of what is universally just (1135a 5–8). Similarly, the theory of justice found in the first part of Book 5 is established without adequate reflection on the natures and ranking of those who devise and are subject to rules of justice and on the intentions of their deliberate actions. Thus it entails the supposition that nature can be disregarded or taken for granted in our calculations. Aristotle, while granting that for practical purposes convention must replace nature in political communities, contends that the just presupposes certain human needs. Given his conclusion that he has spoken of the universally just (1134a 15–16) and given his remark soon thereafter that we must not forget about the search for the simply just and the politically just (1134a 24–26), we might surmise not only that the universally just is not the simply just, but also that one cannot ascend from an analysis of the universally just to the simply just.[10] We cannot evaluate conventions and laws until we know more about the human needs from which they issue. The analysis of political justice which begins at this point does not take for granted that there is a natural support for justice; if anything, it views justice in potential opposition to an intransigent human nature. It may be necessary to conclude that a theory of justice is impossible, because if justice is merely an expedient human creation, then it may have no natural or necessary principles. In what follows, Aristotle will restate the need for law, which serves as the universally just for every city, and then state with a clarity unusual for him, the necessary insufficiency of law because of its universality. He will not speak of the simply just, although he will refer once to "the first justice" and will speak of equity as a kind of justice superior to legal justice. This ascent is made by means of an analysis of the intentions of the doers and sufferers of injustice or unjust deeds. Aristotle faces with more seriousness than most philosophers and citizens the possibility that there is no natural ground for justice and that if this is known to be so, neither should continue to take justice very seriously. He seems to show us that the demand for the simple theory of justice provided thus far has been met at the price of a neglect of an inquiry into nature. Aristotle's own inquiry here does bring to light the problematic truth, which at the same time engenders the gentleness characteristic of his own writings on justice.

Political justice, we are now reminded, subsists among beings whose relations are defined by law. Such legal definition is desirable when one expects that injustice will otherwise prevail, since human beings tend to seek their own good. The rule of a law which distinguishes in a formulaic manner the just from the unjust is preferable to the rule of human beings who would presumably make such distinctions with only their own good in mind. Just rulers, who rule for the good of others, must be compensated with rewards and honors, that is, assured that their good is otherwise provided for (1134a 34–b 8). Thus we must not suppose that rulers will be altruistic or even impartial distributors of justice. The opportunity for tyranny, however, is diminished by the very nature of law, which, in intending to be impartial, leads us to assume that individuals do not differ much or that they are roughly equal (1134a 26–28). Law also incorporates the assumption that human beings are free—free to define their relations by law and to act on that definition or in violation of it. Political justice, then, as opposed to universal justice, does not fail to account for the rank and willfulness of human beings, although it necessarily takes as answered questions which would strike the philosopher as still unsettled. Aristotle shows that political justice rests on at least this much of an opinion about human beings and their possible orders, as well as on the plausible assumption that there is no natural guarantee for order which would make human law unnecessary.

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[10] Jackson (1973, pp. 101–02) plausibly translates this as "what we seek is not merely *to haplōs dikaion,* but also *to politikon dikaion,*" implying, as he makes clear in a note, that the simply just has already been discussed. He nevertheless considers the discussions of political and household justice which follow to be elaborations on the simply just. Gauthier and Jolif (1958–59, p. 386) take the politically just, as distinguished from the simply just, to be justice as it can be realized in the city, as distinguished from an ideal reality. Stewart (1973, pp. 479–80) reads the passage in a similar way. But see Grant (1885, Vol. 2, p. 124). A plausible translation of the phrase *to haplōs dikaion* as "the simplistically, crudely, or naively just" is not offered by any of the commentators. Such a translation would in any case not vitiate my contention that "universal justice" cannot be equated with a justice that satisfies all the demands we make of justice.

If this position is correct, however, then one is confronted with the following difficulty: If justice and the laws that compel humans to act justly are contrary to the natural good of each, then someone may reasonably ask why one should have any respect for those laws, as distinguished from an expedient regard for the strength of those who enforce them. To argue that laws are merely conventional, not natural or authoritative because they exist prior to and beyond human devising, and that obedience to them is merely necessary, not good or choiceworthy in itself, is to undermine whatever disposition toward law-abidingness there might be. From the point of view of politics and its necessities, such an argument could be tolerated only if it were completed by a demonstration that justice, even if depending more on convention than nature, is nonetheless in accordance with nature and that law-abidingness, while necessary, can also be understood as choiceworthy. In what follows Aristotle attempts to make this required demonstration. Whether it is consistent with the truth as he sees it remains to be considered.

It is after his apparent admission that what is just depends on law that we find the only explicit discussion of natural justice or right in all of Aristotle's writings (1134b 18–1135a 5).[11] He insists that the politically just, while conventional, is only partly conventional and is also partly natural. The evidence given in support of the conventionalists' argument was that human laws and conventions vary, whereas natural laws seem to be unchanging.[12] Greeks and Persians have different political regimes and they bow to different gods and in different ways, but fire burns the same in Greece as it does in Persia. In response, one might counter (as some have done[13]) that the variation of laws in different times and places can be traced to an imperfect perception or imitation of the eternal fixed and universal natural principles of justice and, therefore, that the evidence does not preclude the possible existence of natural principles of justice. Aristotle, however, does not make this argument.[14] Rather, he responds that the natural as well as the conventional is changeable: If anything just exists by nature, it will have changeable rather than fixed universal principles. If there is any fixed principle in nature (other than those which might pertain to the gods and which in that case would not be principles of justice),[15] it is that of the best, or the good.[16] Conventional determinations of the just will vary, and if they are to be compared, it must be with respect to their goodness, not their justice. Aristotle says that the natural element in the politically just is that which has everywhere the same capacity; he also says that one regime is the best according to nature. The example of natural changeability is that the right hand is stronger by nature, although it is possible to become ambidextrous. Nature, in giving us capacities, seems to sanction our development of them (perhaps even by means of the laws we make to command virtue). Nature has given us two hands, of which the right is usually stronger. In giving us two hands, nature suggests to us the possibility of going beyond what she has done for us, although not beyond what she herself has shown us. Ambidexterity, the consequence of an unusually gifted nature and training, is both possible and undoubtedly superior to the "universal" phenomenon of right-handedness. Moreover, seeing right-handedness in the light of this possibility enables us to be more tolerant of left-handedness.

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[11] Reference is made to a common law, unchanging because according to nature in *The Art of Rhetoric* (1373b 2–18, 1375a 27–b 5). There, however, the context is the proposed usage of such an argument in forensic rhetoric.

[12] Gauthier and Jolif (1958–59, pp. 392–94) recognize that this argument is directed against the conventionalists. For an analysis of the conventionalists' argument and its significance, see Struass (1953, pp. 97–117). My essay as a whole reflects a far larger debt to the work of Leo Strauss than could properly be repaid in any number of footnotes.

[13] Consider Cicero (1929, pp. 215–16), where the argument is made by Laelius. See also Thomas Aquinas (1948, pp. 640–44). The passage is from the *Summa Theologica* I–II, q. 94 (aa. 4–5).

[14] Ritchie (1894, p. 191) contends, correctly I believe, that "the definite theory of a *Jus naturale* which would apply if there were no *Jus civile* is indeed post-Aristotelian." Thomas, in the passage cited above, does understand Aristotle to say that there are first, as distinguished from secondary, principles of justice which remain unchanging. Hardie (1968, p. 205) also understands that a distinction is made "between principles or rules of justice which would be observed in an ideal community and which accord with the real nature of man and the conditions of human happiness, and, on the other hand, rules observed in some community which falls short of the human ideal."

[15] 1178b 10–18: We do not properly ascribe to the gods acts of moral virtue. As for Aristotle's private opinion on this point, in the passage in which the sign of nature's unchanging character is that fire burns the same in Greece and in Persia, Aristotle neglects to mention that fire was, nonetheless, worshipped as a god in Persia, but not in Athens. (Herodotus, Bk. I, §131).

[16] Although Aristotle criticizes the Platonic teaching about the Good (1096a 11–1097a 14), most of

The demand for a theory of justice is, to repeat, a demand for a precise standard by means of which we can distinguish just from unjust actions. This demand might seem to be met at first by human laws and then by something like a natural law, articulating the universal principles underlying every just legal order. From Aristotle's presentation thus far, we can infer that he does not believe that the demand can truthfully be met in this way. For him, the universality which at first seems so desirable and is the necessary form of the just is, on reflection, not always desirable and in fact, impossible or possible only by means of the tyrannical imposition of human will over nature, because it is contrary to nature.

The demand for a theory of justice, we are told, is also a demand that justice be shown to be a disposition which issues in just acts and intends the just. An examination of the reasonableness of this demand seems to be the theme of the later portions of Book 5.[17] The discussion of natural justice, I have contended, was meant in part to indicate that the best, or the good, might be the standard in the light of which laws and conventions should be evaluated. Human beings naturally tend to act on their opinions about the good, not the just. I believe that what Aristotle attempts to do at this point is to show that reason as well as passion suggests that an exclusive or dominant concern for justice is not the appropriate standpoint from which to view politics. The incidence of just or unjust acts successfully executed and intended as just or unjust is exceedingly rare, and it may be that more actual injustice is averted not by the intention of justice, but by a concern for one's own good, conceived of and pursued in a certain way.

The topics treated in the remainder of Book 5 are the distinction between justice and injustice and the commission of acts with just or unjust consequences (1135a 15–1136a 9), whether one can suffer injustice willingly (1136a 10–1137a 30), the relation of equity to legal justice (1137a 31–1138a 3), and whether one can do injustice to oneself (1138a 4–1138b 11). The first topic, Aristotle reminds us, has been considered once before (1135a 23, 1109b 30–1114b 25).[18] The difficulty is that a moral posture is unintelligible without the questionable supposition that human actions are deliberate or at least voluntary. When we praise or blame or reward or punish someone for performing a certain action, we necessarily suppose that the doer chose it, or at least did it willingly and was free not to do it. More important for our immediate concern, justice: it is surely wrong to punish someone for doing or failing to do what could not have been helped. In Book 3 of the *Ethics,* where there is at most a threat of censure and none of legal judgment and punishment, Aristotle attempts to argue that there is virtually no action which cannot be conceived of as voluntary (1109b 35–1110b 17). He speaks as if we could make ourselves responsible for our natures (1114a 21–1114b 25), although what we usually mean by nature is what is given to human beings in contrast to what they make or do. Here, in Book 5, we learn not only of the numerous varieties of excusable ignorance or miscalculation (1135b 11–24), but also of actions which, while done

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these criticisms are logical criticisms of the Platonic formulation or arguments from contemporary practice which would not be decisive for a philosopher. Cf. Taylor (1818, pp. 12–21).

[17] Gauthier and Jolif (1958–59, pp. 328–29, p. 385) divide Book 5 into objective and subjective points of view. Or as Grant (1885, Vol. 2, p. 102) puts it, the first part deals with the just things and the second with justness. According to Thomas (1964, p. 383), however, the fundamental divisions of the book are justice in the proper sense and, at 1138a 4, justice in the metaphorical sense.

[18] Aristotle's treatments of spontaneous actions are usefully compared in Faulkner (1972, pp. 81–106). Cf. Gauthier and Jolif (1958–59, pp. 397–405).

knowingly, are neither voluntary nor involuntary, but *natural,* as are growing old and dying (1135a 33–1135b 2). One purpose of Aristotle's argument seems to be to show us how difficult it is, given the absence of universal rules of actions, to determine the just act in each situation (and, therefore, how much we might excuse). We are also shown how few legally just acts originate in the intention to be just (and, therefore, how contemptuous we might be of the opinion that justice is law-abidingness). Just acts done out of fear of punishment are not essentially just (1135b 4–6). And injuries done not with deliberation and choice, but out of anger or other "necessary or natural" human passions are not essentially unjust; many such acts are, in fact, reactions to supposed injustices (1135b 19–1136a 1). Presumably when there is general agreement that the supposed injustice was an injustice, among these reactive injuries are included acts undertaken in enforcement of the law, especially punishment of offenders. These acts are neither essentially unjust nor just, because they originate in passion or anger rather than deliberate choice (and are probably not choiceworthy in themselves).

Our increasing disdain for the opinion that justice is law-abidingness is mitigated only by our cognizance, awakened by the next set of questions posed by Aristotle, that to act justly at all is exceedingly difficult. Common opinion has it that justice is a habit or disposition in contrast to a science (1129a 11–16). While Aristotle does not hold that justice is a science, he surely shows that it is dependent on intellectual virtue of some sort (1137a 4–26, 1144b 28–32). How different the requisite intellectual virtue might be from what we now usually think of as political theory or science, is indicated by the fact that instead of being mathematical in form, as was the first portion of Book 5, this later portion makes the questions raised by the poets thematic. Aristotle uses the example of a man who ignorantly kills his father (as did Oedipus, thereby fulfilling his destiny) (1135a 28–30); he attempts to answer Euripides' question as to whether someone can willingly suffer injustice (as Alcmaeon's mother might have done in order to relieve her son of the burden of a curse) (1136a 10–16); he reports Homer's account of Glaucus' giving away his treasures (because deluded by the gods) (1136b 9–11). He seems to concede tacitly that the poets would be correct in elaborating on the inevitable doing and suffering of injustice if the knowledge necessary for justice were withheld from us by the gods or nature. The requisite knowledge, however, seems to be knowledge of the passions "necessary or natural" to human beings.

The poets, who teach us about the passions necessary or natural to human beings and the ones which are "neither natural nor human" (1136a 7–8), ask "strangely" whether one can willingly suffer injustice (1136a 10–15). Despite Aristotle's assertion that the question is "strange," it is less strange than it might seem if we recall that Aristotle has insisted that moral virtue must be a mean between two vices (1106b 36–1107a 2), and vices are necessarily voluntary. Justice then, must be thought of as a mean between willingly having too much and willingly having too little (1129a 3–5, 1133b 30–1134a 14). But justice, as the practice of virtue toward others, always involves the willfulness of those others for whose actions we cannot be responsible. Our not having enough might be a consequence of others' not giving enough, not of our being unwilling to take enough. Their wills inevitably limit our actions, but to make the intentions and actions of those to whom we would be virtuous a part of our calculations is fraught with complication, if not absurdity. This point Aristotle makes first in an argument about suffering injustice. Even if we were willing participants in unjust or just acts, we could not will to be treated unjustly, because injustice, as distinguished from the commission of unjust acts, depends on the will of the doer. In a second argument he contends that one could not even willingly suffer an unjust act, because everyone intends the good, but an unjust act is presumably a harm to the sufferer. Thus at the same time that he shows the difficulty in attempting to make the good of others the ground of one's actions, Aristotle reminds us that the natural ground of all actions is a concern for the good, or the serious (*spoudaion*) (1135b 7–8), which is not necessarily one's own good capriciously willed or the good of others as they see it.

Although it *appears* possible to suffer injustice willingly, this cannot be so, according to Aristotle. For to call it a matter of injustice, we must be able to say that the unfair distribution of goods or harms was voluntary. Yet no one wishes to be harmed–by oneself or by anyone else. Must we then somehow understand the phenomenon as that of the apparent sufferer's viciously intending to take too little for himself? We might say that here the individual neither suffers injustice nor inflicts it on himself, because the disdain for a fair share of apparent goods is a consequence of the intention to secure greater, if less tangible, goods–reputation or nobility simply (1136b 21–22). The strange phenomenon of someone who seems to suffer injustice willingly is in fact the phenomenon of someone who desires and pursues goods about which most people, who seek wealth, do not think. This individual might well be guilty of injustice in a double sense: giving others more economic goods than is fair and selfishly appropriating another kind of good. No one would be convicted on the first count, because the definition of injustice has now been revised to specify that the act must be contrary to the wish of the sufferer,[19] and few people unwillingly accept an undeservedly large share. But even if no legal complaints were filed, and even if the wrong judgment were given unknowingly, there would be a violation of "the first just" (1136b 32–35).[20] It is at this point that Aristotle remarks how hard it is to be just—to act knowingly and with the intention of distributing fair shares. If the law compelled such actions and distribution, that would be accidental (1137a 11–12); law and legal justice do not ensure a wise distribution of all goods or of "the simply good things" (1137a 26–27). Our immediate difficulty, however, is that it seems paradoxical to conceive of men usually praised for their restraint or equity as unjust,[21] or as men who knowingly and willingly harm others. I think we must acknowledge that the teaching about justice presented thus far does not satisfactorily account for the interesting, albeit rare, phenomenon of the man who seems to suffer injustice willingly. An attempt to do justice to this phenomenon leads to the critique of universal justice which follows in Aristotle's text.

We have begun by assuming justice to be good because it is a virtue, if not the whole of virtue. The just, we have seen, is necessarily embodied in laws and conventions which articulate the rule of the usual or average case. At the same time, we often say that equity (*epieikeia*) is good, praising it even more highly than justice. The equitable man characteristically does not demand his just share, taking less than he is legally entitled to (1137b 24–1138a 3). But such a man, we have concluded, is unjust.

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[19] Gauthier and Jolif (1958–59, pp. 414–15) point out that the qualification of the definition of injustice at 1136b 3–5 with the addition of the phrase "against that person's [the sufferer's] wish" is in striking contrast to Aristotle's usual qualification "against the law," citing 1138a 8 and *Rhetoric* 1368b 6. They do not observe that the formula for vice in general should be "against the right rule or reason [*logos*] as the prudent man would define it" (1106b 36–1107a 2) or simply "against the right rule or reason" (1133b 18–21). The qualifications "against the sufferer's wish" and "against the law" could be reconciled if laws were—as they often are in republican governments—made according to the wishes of like-minded people. The absence of the qualification "against the right rule or reason" merely underscores the problematic character of justice as a virtue because of its necessary dependence on law and politics.

[20] What is meant by "the first just" is most unclear. According to Gauthier and Jolif (1958–59, p. 419): "Le 'juste au sens premier,'—on reconnâit la saveur platonicienne de l'expression—c'est le juste non écrit (*agraphon*), naturel (*phusikon*), par opposition au juste legal et conventionnel (*nomikon*), que suppose une intervention humaine.... L'idée de justice est plus exigeante que toutes les determinations humaines...." Given that Aristotle never speaks of an idea of the just, this explanation may be to find too much of a "saveur platonicienne" than is warranted.

[21] Out of respect for the common love of justice, I have conformed to the new requirement that half the human race not be slighted unfairly or unnecessarily because of thoughtless use of the English language. Out of respect for philosophy, I have attempted to make my language consistent with Aristotle's. What has traditionally been rendered as "man" in English can be "*anthrōpos,*" "*aner,*" or an adjective used as a noun in Greek. *Anthrōpōs* refers to the human species, but *aner* is emphatically "male human being." Perhaps to be consistent in his point that law and justice tend to abstract from individuals and natural human differences, Aristotle uses *aner* only twice in Book 5 (aside from one quote). Remarkably, he does so to speak of "the good man," whose education may not be supplied by a mere citizen's education (1130b 27–29), and of "the equitable man," whose justice is superior to legal justice (1137a 35). Generally in Books 5, 8, and 9, he uses *anthrōpōs* to indicate the human species, as distinguished from brutes and gods (1129b 4, 1134a 35, 1135a 4, 1135a 29, 1135b 22, 1136a 9, 1137a 30, 1155a 18, 1155a 20, 1155a 22, 1155b 9, 1159a 10, 1161b 6, 1161b 8, 1162a 5, 1162a 17, 1162a 20, 1163b 24, 1169b 18, 1170a 17, 1170b 13), in political contexts (1168b 33, 1169b 18, 1170b 3), and in a somewhat derogatory tone (1129b 4, 1134a 35, 1135a 4, 1137a 5, 1157a 25, 1167b 27). In Books 8 and 9 he uses *aner* chiefly in contrast to female human being, a contrast which is required because man and woman are to be distinguished according to their characteristic "works" or functions, even as they live together as friends. *Aner* is used at 1158b 13, 1158b 17, 1160b 32, 1160b 33, 1160b 34, 1160b 35, 1161a 22, 1162a 16, 1162a 22, 1162a 30, 1164a 27, 1165b 27, 1171b 6–11. The rule of male over female is fitting and just, *assuming* that the male is superior in virtue (1161a 22). That the distinctions may ultimately pertain more to soul than to body is acknowledged at 1171b 6–11, but Aristotle often deferred to his readers' prejudices for the sake of persuading them on more important issues. His own high regard for the virtues of women is revealed in the *Politics* at 1260a 20–24, 1260b 8–20, and 1277b 16–25.

Justice and equity, therefore, seem to be opposed to one another, and we might wonder how two opposed things can both be good. Aristotle says that we must reconcile the two by understanding equity as something which, although different from legal justice, is another sort of justice, not generically different from it (1137a 33–34, 1137b 33–34). We understand it as a necessary correction of legal justice and superior to it; in so understanding it, however, we acknowledge the necessary insufficiency of law or of any statement of the universally just (1137b 17–19). We demand a universal measure of the just, but if there is to be such a measure, it must be not a rigid rule, but a flexible one as the Lesbian builders use (1137b 29–32). Even the legislator, were he present, would correct or suspend the law in the exceptional case (1137b 19–24), but Aristotle says nothing about the kind of principle that might inform the legislator's judgment. Art, we are told soon thereafter, is necessarily concerned with universals (1138b 2–4). We might wish to think of the necessary principle as the sought after "just simply," but Aristotle does not mention it at this likely point. In any case, Aristotle never says that equity should replace rather than supplement legal justice. We are left with the necessity of reconciling a universal with a particular which is distinct from and opposed to it. I do not believe that this theoretical difficulty is resolved in the context of the teaching about justice in Book 5.

Should we wish to dismiss this difficulty as merely theoretical or abstract, Aristotle brings home its practical significance in his concluding discussion, which is a return to the question of whether one can ever be unjust to oneself.[22] The answer given is a "no." Injustice as willful disobedience of the law cannot be conceived of as injustice to oneself because, being injustice, it must be voluntary, but being voluntary, it cannot be a harm to oneself, because human beings do not willingly harm themselves. For this and other reasons, neither can injustice in the partial sense be conceived of as injustice to oneself. One can harm others, however. Aristotle's principal example of an unjust act is suicide. Justice, we recall, is the practice of virtue toward others. Since the purpose of the law is to encourage the virtues, the refusal to obey it is an injustice to the city, properly punished by the city. Yet suicide, or its consequence, death, we can also understand as the greatest harm that can befall a human being, for it is probably the ultimate denial of opportunity to practice toward anyone the virtue which is also the condition of one's own good, or happiness. If it can be demonstrated that in committing suicide one does not do an injustice to oneself, then we must conclude that justice is concerned *only* with the good of others, not one's own good. Yet at the same time, Aristotle continues to insist that human beings do act in pursuit of a good in which they can share.

The injustice of suicide is traced to the principle that what the law does not command, it forbids. Here we might remind ourselves of Socrates' philosophizing and see that it, too, in not being commanded by the law, was forbidden (Plato, *Apol.*, 29c–d). Rather than to cease his illegal philosophizing, Socrates chose to die, or, in effect, to commit suicide. The failure of the law to command or permit the perfection of his intellectual virtue left him no choice but to act illegally, hence unjustly, for the sake of fulfilling the intention of the law. The two concluding points in Aristotle's exposition of justice are, first, that to do injustice is not always the greatest evil (1133a 28–b 6), and second, that one can speak of doing justice to oneself only metaphorically (1133b 7–13). Thus for the respectable to grieve for Socrates is tantamount to an admission that they do not in truth hold justice to be the most serious thing in life or even in politics. Justice, even if it is the whole of virtue to others, should not be mistaken for the whole of virtue, moral and intellectual. Only in friendship is the practice of the whole of virtue necessary and possible.

The demand for justice and for a theory of justice must be met, because such a demand originates in a "necessary or natural" human passion; but the political philosopher above all ought to perceive that the theory which meets this demand does not satisfy all human demands, especially those of reason. Aristotle shows us, therefore, that the demand is somewhat misguided. First, as friends of the truth, we must acknowledge that the opinion that a theory of justice is possible, because there are natural and necessary universal principles of the just to be grasped, is based on unexamined assumptions about nature. Insofar as the theory does implicitly posit an understanding of human nature it is that human beings intend and act in pursuit of their own good. In effect, then, the theory bids us to act contrary to our natures—for the good of others, so we might doubt the efficacy of the theory as a moral imperative. Finally, its benefits would be limited, although not insignificant. Laws and universally applicable rules would help to ensure that the good of the nasty and excessively greedy, especially their good conceived in anger and haste, is not bought at the price of the suffering of the weak. But the universal rules provide no guidance for the resolution of the greatest political problems, and in strengthening our tendency to neglect exceptions, they blind us to certain things which might be worthy of our most serious attention. Aristotle's critique of his own attempt at a theory of justice points to the conclusion that our demand might be better served if shown to point elsewhere than to justice. What we really demand is a theory which, although undeniably valid, is not necessarily universally applicable; which, consistent with the necessity of human nature, intends a good in which all doers as well as sufferers of deeds can readily perceive their share; and which, nonetheless, does have just consequences.

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[22] Most commentators find the presence of the last chapter of Book 5 problematic. Grant (1885, Vol. 2, p. 141) calls it superfluous, observing that "there is no merit to the present discussion. Amidst the feeble reasonings and the repititions which it represents, the only points the least interesting are the view that is taken of suicide, §§2,3, and the saying that it is a mere metaphor to speak of justice between the higher and lower parts of a man."

At this point, we should briefly recall the other comprehensive virtue of character, pride, or greatness of soul (*megalopsychia*) (1123a 34–1125a 35). Whereas justice is said to be the whole of virtue toward others, pride is said to be the ornament (*kosmos tis*) of the virtues. It presupposes all of the virtues and makes them greater. The proud man or woman deserves great honors and does not fail to claim them. Being deserving of honors, he or she possesses the virtues, because only virtue is worthy of honor.[23] Pride and claims to honor make the virtues greater and ornament them, we might surmise, because to be conscious of virtue as virtue is to possess an element of intellectual virtue in addition to moral virtue (1144b 1–4). The extreme which is most opposed to pride is pusillanimity, and this is more an error than a vice, for not to make claims for one's own virtue is not to know oneself and one's worth. Furthermore, we might suppose that should the law fail to command all the virtues, the proud individual, insisting that, being virtuous, he—like the gods—is worthy of honor, presents himself as a model of virtue for us to admire and imitate. He is *justly* contemptuous of inferiors and shows his hates and loves openly, although he is not incapable of ironic self-deprecation. While accepting honors, he nonetheless disdains them, because to take honors seriously would be to judge his own worth by the estimates of the inferiors who accord him honors. The only one he could live for would be a friend.[24] Yet he willingly helps and benefits others, because as a benefactor, he can take pleasure in demonstrating his superiority. For the same reason, he is loathe to ask for help and does not like to recall benefits conferred on him, although he will repay them lavishly. He acts infrequently—only when the occasion requires the exercise of great and rare virtue.

The theme of Book 8 and 9 of the *Ethics* is friendship. My purpose here is not to offer a comprehensive analysis of Aristotle's teaching on friendship, but to make some observations and speculations about the place of that teaching in his political science. In particular, friendship and justice are said to be concerned with the same things and persons. Indeed, we are now told that friendship holds cities together, that legislators are more serious about it than justice, and that while friends do not need justice, the just still need friendship (1155a 22–28). Here, from the perspective of friendship, the problems mentioned in Book 5, but not developed or left unresolved, are developed and suggestions for their resolution made. We might infer that in proceeding as he does, Aristotle means to show us that the problems that arise in politics can be solved only in the spirit of friendship, trust and good will, not in the spirit of punitive justice or even impartiality. Furthermore, in calling our attention to the everyday phenomena in which the presence of trust and good will cannot reasonably be denied, he gives the evidence that those who assumed nature's beneficence failed to give and he shows to those who would deny it, that something like justice, or virtue toward others, does have a ground in human nature.[25]

By means of an examination of friendship, Aristotle attempts to make intelligible all human associations, both among human beings and within human beings, who have composite natures, and perhaps all associations, or wholes.

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[23] *Aner* is not used in the discussion of the magnanimous individual.

[24] The noble individual is said repeatedly to desire honor in order to confirm his or her opinion about the existence and worth of his virtue. In Book 8 (1159a 12–27) he is shown that the affection of a good human being who befriends him serves this need as well as better, so the honor accorded the good could be said to be as useful to others, as a reminder of what virtue is, as to the noble.

[25] See p. 1205.

More precisely, he makes them intelligible by comparing and contrasting them to a perfect form of friendship, which is possible only among the good, brought together on the basis of their virtue. Because this form of friendship is comprehensive as well as superior, all inferior forms of association can, when measured by it, be seen as partial or defective forms. Thus they can be seen not only as what they are, but as what they are meant and fail to be. Whoever thinks he intends the just really intends friendship, and if the city cannot secure that friendship, it is properly depreciated.

Friendship is some virtue or is with virtue (1155a 3–4). It begins with good will, but is sustained by mutual affection or passion (1155b 27–1156a 5, 1166b 30–1167a 21). Each human being has affection for what seems good to him or her (1155b 23–25), and what seem to be good are the good, the pleasant, and the useful (1155b 18–19), so friendships can exist for the sake of any of these three ends. The good, who love the good, befriend others like themselves because of their goodness. If to be good is good for human beings, then in loving a friend as good, one not only loves him or her for himself or herself, or essentially, but one promotes his goodness, and thus his good, for a friend will prize the affection which is affection for his goodness (1156b 7–11, 1159b 4–7, 1170a 11–13, 1172a 8–15). Since a friend becomes dear to oneself, one secures one's own good in intending his or hers (1157b 33–35). Since the friendship of the good is also pleasant and useful to both parties (1156b 13–15, 1157a 1–3), their association secures to both the comprehensive good or happiness that the law claims to secure to political communities.[26] Because their association is by a choice, born of disposition as well as passion (1157b 28–32), it will be stable and long-lasting without the convention and law needed to stabilize associations for utility or pleasure.[27] The good who are friends can and do trust one another (1156b 28–29, 1157a 20–24), so injustice need not be anticipated and the institutions and procedures to minimize it need not be established. Such friendships, "complete in time and all other things" (1156b 33–34), are rare, not only because good human beings are rare, but because trust is the product of long familiarity or, we might say, of knowledge of the nature or character of one's friend[28] (1156b 25–29). Indeed, good friends must spend time together, living together and sharing activities by speaking together of them (1159b 7–13, 1157b 19, 1158a 10, 1171a 8–10, 1171b 29–1172a 15). Now if it is correct to define human life by the active exercise of the capacities of perception and intellection (1170a 16–19), then it seems correct to say that in such a friendship one actualize one's being, and life, especially the life of a good human being, is good for him or her and perhaps good simply (1170a 25–1170b 19). Thus it can be said that perfect friendship satisfies the natural, reflective concern for one's own good. Perfect friendship begins and ends with the love of the good in which one necessarily shares.[29]

Aristotle begins to make the comparison of politics to friendship explicit in the context of a consideration of friendships among unequals, in which one friend is superior to the other in some respect (1158b 11–14, 1159b 25–26). To raise the question of how such relationships are to be equalized is to raise the question to which different political regimes are answers.[30] Here, Aristotle speaks not of a natural law, or even of a natural justice, but of the *justs,* which are not only conventional, but variable (1159b 35, 1162b 21–23). Nevertheless, he moves from the first acknowledgement of unequal friendships to the explanation of political regimes by means of a statement about the kinds of familial relationships (1158b 15 ff.), thereby implying that the various determinations of the just are imitations of natural models. Political communities, however, are said to be less just than families because strong natural affections among citizens are lacking (1160a 3–8). Aristotle revises his famous dictum, asserting that by nature the human being is more a conjugal than a political animal (1162a 17–18). The lack of natural affections does not seem to constitute the essential flaw in the political association, however, for the discussion at the beginning of Book 9 about how to rank differing and conflicting obligations (1136b 30–1165a 36) serves to remind us that the friendship of the good requires an element of deliberation and choice which is lacking in a mother's instinctive love for her child. This same discussion of friendships among those who are not only unequal, but who have differing ends, does bring to light the almost inevitable defect of political associations. Those with differing ends are exemplified by philosophers or teachers as opposed to students who think that philosophers care about the money they are paid for teaching (1164a 22–1164b 6). The political association is a friendship for utility (1162b 22–25), not for pleasure or the good. Although natural affections, characteristic of the family, or patriotism in the city might diminish the controversy which is characteristic of friendships for utility (1162b 5–6), bickering over the distribution of goods and over payments, monetary or honorific, for services rendered is virtually inevitable, since "all or most men intend the noble, but choose the profitable" (1162b 34–36). Noble friends, the equitable who often do not claim their due, demand not the just, but the possible repayment (1163b 15). Noble friends, furthermore, determine worth and, therefore, payment due by the intention of the first giver, but in politics worth must be judged by the recipients' estimates of their own needs (1163a 16–23).

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[26] See p. 1203.

[27] See p. 1206.

[28] See p. 1206.

[29] See p. 1211. Hardie (1968, p. 335) is to be commended for the recognition, all too infrequent today, that one can be a moralist without being a Kantian: "Moral obligation is not a simple concept of which there is an agreed account so that we can ask whether Aristotle had the concept, yes or no. If we ask in what shapes the experience or fact of obligation came into his view we should consider his use of 'ought' (*dei*) and of 'right' (*dikaion*) but also what he calls the 'noble' (*kalon*). This is something which 'we divine to be proper to a man and not easily taken from him' (I.5, 1095b 25–26). His doctrine of the final good is a doctrine about what is 'proper' to a man, the power to reflect on his own abilities and desires and to conceive and choose for himself a satisfactory way of life. What 'cannot easily be taken from him' is his power to keep on trying to live up to such a conception; to obey, as Aristotle says, his 'reason' (*nous*). It may indeed be admitted that Aristotle did not distinguish sharply, as Kant tried to distinguish, between the rationality of the moral law and the rationality of 'enlightened self-interest' or, to use Aristotle's word, 'self-love.' "

[30] See p. 1204.

With some sense of what seems good for themselves, most people try to understand the city as either a business partnership or a charitable institution. Oligarchs contend that the distribution of goods must be a distribution of profits commensurate with contribution to the common wealth; democrats contend that the distribution must be commensurate with need. Aristotle proposes the reconciliation of these two correct, but opposed, views in what is, in effect, aristocracy or kingship (1163a 24–1163b 12). Those who contribute wealth and virtue are to be recompensed with honors (not wealth), and the poor are to be given what they need (wealth) and denied what needy people do not deserve (honors and political offices). In politics, honor replaces the affection said to equalize friendships between unequals. In Book 9 we learn that concord (*omonoia*), or political friendship, is most of all the agreement of the demos and the equitable men that the best should rule (1167a 35–1167b 1). (An example of the rule of the best is the rule of Pittacus, a wise man.) Aristotle says soon thereafter that such concord is found among the equitable, but not among the base (1167b 4–12), so we might surmise that if concord is maintained at all, it is in a regime in which the equitable hold office without being dependent on election by the base. What makes a political solution possible at all, although not inevitable, is that there be capable and fortunate individuals who understand their good in a way most people do not and that the distinction between need and desert or between base and noble needs be maintained in an obvious way. Although Aristotle's theory of justice does not preclude aristocracy and constitutional monarchy, that theory, which relies more on impartiality than equity, lends little support to the necessary distinction. Aristotle's lengthy explanations of pride and of friendship based on knowledge of the superiority of oneself and one's ends do support it. In other words, according to Aristotle, a just and good political regime is the consequence not so much of making justice one's end as of acknowledging a rank order of human needs and the human beings who exhibit them.

Given this understanding of the conditions required for the solution of practical political problems, Aristotle would deem it essential that political scientists speak as he does of friendship as well as justice. In practice, friendship is meant to be a supplement to justice. Because perfect friendship is rare and not universally practicable, Aristotle teaches about justice and never denies the necessity of the kind of justice in which his theory of justice tends to issue. But in theory the teaching on friendship is, I believe, meant to replace rather than supplement the theory of justice attempted in Book 5 of the *Ethics*, because it grounds in a more satisfactory way the institutions and habits which are necessary to ensure a modicum of justice in cities. We might suppose that those least likely to accept the imperfect theory of justice would be potential tyrants, who need to pursue their own good as they see it, and philosophic natures, who need to follow the dictates of reason.[31] Aristotle's best response to the first is a rhetorical display in which friendship is made so attractive that they will perhaps come to see friendship, and not tyranny, as their good. His response to the second is an argument to the effect that if one reflects on what it means to be a human being, one must grant that the human good is realized in the most perfect friendship. Most might well choose not to act on Aristotle's teaching, but because of it, they will be hard pressed to give a rational defense of their choice. If friendship is the human good or makes possible the human good, if friendship requires virtue, and if virtue is a disposition or habit acquired with the aid of laws that compel to some extent virtuous actions (1179b 20–1180a 24), then those concerned about their own good have a reason for obeying and defending decent laws. For the most part, they could choose to act in accordance with them. Rather than proposing that friendship replace justice, Aristotle uses the teaching on friendship to justify the most comprehensive laws. Otherwise, the city and its justice are indefensible.[32]

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[31] Cf. *Politics,* 1266b 38-1267b 17. Potential tyrants, the perpetrators of the greatest injustices, need a special education. That education is an exposure to the pleasures of philosophizing.

Politics rarely, if ever, transcends itself, but it can always be understood by statesmen and political scientists in the light of what transcends it. Seen in the light of friendship, the city is an imperfect friendship, and its good, justice, is not the whole human good. This point of view presupposes an examination of human nature, of its rare, but possible, perfection as well as of its "universal" imperfection.[33] Nature is neither disregarded nor taken for granted. By the necessity of human nature, friendship is necessary, although what Aristotle means by necessary is not that it is inevitable, but that it is the condition of human being, properly understood (*Physics*, 199b 33–200b 9). No reasonable individual concerned with his or her good as a human being would fail to choose it. But friendship is also noble and choiceworthy (1155a 28–29), and far from being inevitable, it must be intended and chosen, and certain activities must be undertaken to sustain that choice. At the conclusion of Book 9, Aristotle contends that the choice would be made whatever fortune might befall one, as if to say that, practically speaking, no more of nature or the gods need be known.[34] In this way, Aristotle's political science can be said to be precise and comprehensive, even if it offers no universal formulae for justice.[35]

Political philosophers and philosophers today assume that their task is to formulate a theory of justice, which will result in justice in practical politics. Their theories, which they suppose to be in accordance with reason or science, may in truth have no real basis in any fact other than an incoherent human demand for justice. In the *Nicomachean Ethics* Aristotle implicitly contends that the attempt to formulate a satisfactory theory of justice leads us to see for ourselves that what we desire when we demand justice and formulae for justice, and therefore a general political solution, is properly supplied in a friendship of the virtuous, which is a rare, but possible, nonpolitical solution. Our expectations from politics are properly diminished as a consequence of our deeper reflection on human nature and on the limits of politics. In looking at politics in a way that brings to light its necessary imperfection, Aristotle is nonetheless able to justify politics and its justice as what they are, as well as to show how his taking justice less seriously may well lead to more equity in practice. We are led by Aristotle to wonder whether the attempt to transform philosophy and politics into the search for perfect justice is not intellectually futile and politically harmful. The current rage for theories of justice may well be an unfortunate, if well-intentioned, manifestation of the passion for justice.

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[32] See p. 1207.

[33] At the beginning of Book 7 (1145a 15–33), a "new beginning" is made. That beginning is an examination of the full range of human natures, divine, human, and bestial. The phenomena of virtue and vice are reconsidered in this context.

[34] At the beginning of the examination of friendship (1155a 32–b16), Aristotle acknowledges that some try to find a "deeper and more physical (natural)" explanation of friendship in non-human nature. While Aristotle puts aside such inquiries, he reopens them briefly (1159b 12–24) and later himself offers two "more physical" explanations (1167b 28–29, 1170a 13–14). The account of human being in Book 9 is followed by an argument in Book 10 about what it means to be.

[35] At the conclusion of the *Ethics* (1180b 20–23), the legislator or student of politics is said to need knowledge of sone universals "perhaps." Nothing is said of justice, howver.

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Machiavelli's Unchristian Charity*

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The central theoretical chapters of the Prince *(chapters 15 ff.) yield forgotten justifications of the road the West has traveled. Machiavelli relieves us of our qualms; in chapter 16, over dangling the carrot; in chapter 17, over brandishing the stick. He thereby lays the moral foundations for societies based, as all modern societies have been, on collective aggrandizement recognizing no limits in principle, and on justice understood as the enforcement of "equal (but otherwise boundless) opportunity."*

Many scholars join in calling our attention to chapters 15 through 19 of the *Prince.* These chapters begin with a proclamation of the radical newness of what is to follow. They comprise Machiavelli's explicit statement of what is novel in his political teaching.

The question of the newness of Machiavelli is also that of our indebtedness to him. Almost everyone will agree that Machiavelli achieves the transition in theory from "the Middle Ages" to something less dank. The disagreement is as to how far and in what way.[1] It is to this latter question that I will first address myself, relying on Machiavelli's own statement on it. I will then move on to consider two striking instances—the first two, in fact, which he sees fit to present us—of the implications of his new political science. In focusing on his presentation of the virtues of liberality and compassion, I will argue for the surprising closeness of his treatment of virtue to the treatment of it which is characteristic of economic liberalism, of which these two virtues remain the moral core.

I will consider three well-known chapters of the *Prince* (15–17). These form a coherent argument (as well as making part of a larger one) on which my case will rest. I have not mistaken this argument for the whole of the *Prince,* much less for the whole of Machiavelli. It is, however, one which in the perspective of hindsight looms large among his many arguments. Nor does any other passage of Machiavelli, of comparable brevity, so neatly indicate the relation in which his new perspective stands to the classical and Christian ones, or the route by which he beckons us to pass from them to it.

I

Chapter 15 of the *Prince* signals more clearly than any earlier one Machiavelli's break with the thinkers who preceded him. Its title ("Of those things for which human beings and especially princes are praised and blamed"[2]) promises a treatment of the question of human (not merely princely) virtue and vice, a competitor therefore of Aristotle's *Ethics* as well as of certain sacred writings. Machiavelli begins by declaring that in discussing the modes (*modi*) appropriate to princes, he fears that he will be accounted presumptuous, the more so in that in discussing them he will depart from the orders or ordinances (*ordini*) of "the others." By "the others" he apparently means all of the "many [who] have written on this subject," and by alluding to *modi ed ordini* he implies that what he offers us here is something wholly new and original, comparable to the revolution worked by Moses for the Israelites.[3] Where others have supplied merely an "imagining," Machiavelli will provide the "effectual truth of the matter" of how human beings should conduct themselves.

> And many have imagined republics and principalities which have never been seen or known to exist in truth; for there is such a gap between how one lives and how one ought to live that he who abandons that which is done in favor of that which ought to be done learns rather his ruin than his preservation; for a man who would make in all things profession of

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[1] For an elegant if not always reliable précis of the secondary literature, see Berlin (1972); and for critiques on a grand scale of eight leading interpretations, the monumental French dissertation of Lefort (1972). The principal more recent contribution is that of Pocock (1975).

[2] Translations are mine: I used Machiavelli (1972).

[3] Compare the uses of these terms in the *Prince,* 6, 7, 26; cf. Whitfield (1955, pp. 34–35, 38).

> goodness must come to ruin among so many who are not good. And so it is necessary for a prince, if he would maintain himself, to learn how not to be good, and to use this knowledge or not as necessity demands.

Whatever Machiavelli's innovation, this is not it. There would not have been, as is so often assumed,[4] anything new in the discovery of a wide discrepancy between the purest goodness and the qualities most conducive to political success. "The fact that the wicked flourish . . . has never been very remote from the consciousness of mankind."[5] We need only consider the impact of the fates of Socrates and Christ upon the traditions that sprang from them in order to see that this "Machiavellian" insight had defined the central moral problem with which Machiavelli's predecessors had wrestled.[6] It is, in fact, for just so long as Machiavelli preserves the distinction between the morally best and the politically necessary that he remains within both the classical and the Christian traditions. Only *after* he has stated the discrepancy between thorough goodness and political success does Machiavelli announce that he will "leav[e] . . . aside the things imagined concerning a prince." It proves to be among them.

Machiavelli proceeds to furnish a list of pairs of "qualities" (here as in the title of the chapter he refrains from calling them virtues and vices) which princes are "thought" to possess as a consequence of which they reap praise or blame. Since, he concludes, it is unfortunately impossible for a prince to cultivate all of those qualities that are considered good,

> [he] must be prudent enough to know how to avoid the infamy of those [qualities] that would deprive him of the state, and to keep himself from those that will not deprive him of it, if possible; but if he is unable to do so, he can with less scruple indulge them. And furthermore he will not worry over incurring the infamy of those vices without which he can only with difficulty save his state; for if one considers everything thoroughly, he will find that something that *appears to be* a virtue, if he follows it, will prove his ruin, and that something else that *appears to be* a vice, if he follows it, will win him security and his well-being (emphasis added).

Only in these last lines of the chapter does Machiavelli unobtrusively reveal the full extent of his presumptuousness. This consists not in affirming what no political thinker had ever doubted, but in denying it. There is no gap between the demands of politics and the demands of virtue—between "what is done" and "what ought to be done"—but only between the demands of politics and what appears to be virtuous but is not. Whatever politics demands is virtuous. The touchstone for pretended virtue is its contribution to our security and well-being.

Machiavelli is often defended as departing (and counseling departure) from the older morality only with the greatest reluctance and in a spirit of tragic resignation;[7] as distinguishing between private matters to which this morality is fully applicable and public ones to which unfortunately it is not;[8] as substituting for a narrow or priestly morality the more comprehensive and worldly one of patriotism and the common good.[9] These interpretations have in common that they vindicate Machiavelli's critique of morality by making him a deeply moral man. In opposition to them we must repeat that Machiavelli's novel contention is not that worthy ends may require doubtful means, nor again that then "the end justifies the means." Both philosophy and theology had recognized, without exulting in, the frequent necessity of sacrificing virtue to virtue's interests. Virtue requires society or the Church, but is insufficient to found and to maintain them. It depends on conditions which it cannot guarantee.[10] Still, to do nothing vicious save in defense of virtue is to acknowledge the authority of virtue. Political success, though of greater urgency, remains inferior in worth to virtue, which both requires and transcends it. This is what Machiavelli here denies. The lukewarm moralists who have always flocked to him are rallying to something which he has left behind him. It is true that he frequently exaggerates both the dogmatism of his predecessors and the depth of his own attachment to certain ends (e.g., Italian patriotism[11]) which, because they are capable of passing for moral, are capable of excusing much nastiness. He knows that he will have many readers who, lacking the courage of both their goodness and their badness (cf. *Discourses* I, 27) will willingly indulge immorality so long as they can tell themselves that it is reluctant, undertaken out of necessity or in the service of some higher cause. As the history of Machiavelli's reputation will confirm, such readers have always comprised by far the greater part of those sympathetic to him. He retains, therefore, in some conspicuous places, some of the trappings of moralism. But he invites "those who know" (as in the present chapter) to make their way to seeing these for what they are, to see through them while at the same time grasping their necessity.

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[4] A. Gilbert (1968, pp. 77–83); Anglo (1969, pp. 71–73, 189–90, but cf. p. 190 ff.).

[5] Berlin (1972, p. 150). He adduces numerous biblical and classical examples.

[6] Consider Thucydides V, 84–96, VII, 86; Plato, *Apology of Socrates, Gorgias* (esp. 508c–513c, 521b–522e), *Republic* 517a; Xenophon, *Education of Cyrus* III, i, 38–40; *Memorabilia* IV, viii, 4–11; *Apology of Socrates* 28; Seneca, *On Providence;* Epictetus, Fragment 13 (Oldfather, 1928, pp. 456–59) from Stobaeus, *Florilegium* I, iii, 50; Plutarch, *Lives* of Phocion and Cato the Younger, Dion and Brutus; Boethius, *Consolation of Philosophy*. These examples extend from the dawn of classical political philosophy to its twilight; dozens more could have been cited.

[7] Alderisio (1930); Wolin (1960, pp. 224–28); Russo (1966, pp. 47–48); and, the most extreme such statement, Muralt (1945).

[8] Croce (1925, pp. 60–65); Meinecke (1957, p. 33); Maritain (1942); Russo (1966, pp. 214–15).

[9] This argument approaches the previous one, from which it is not always clearly distinguishable. Very broadly, however, the writers cited in the previous note emphasize on Machiavelli's behalf the insoluble discrepancy between politics and morality, while those whose names follow present devotion to the public good as a higher morality which supersedes the lower one. For obvious reasons this interpretation flourished in Italy for 20 years of the present century, as it had during the middle of the last one. De Sanctis (1912, II, 51–112); Ercole (1926); Collotti (1939); Bizzarri (1940); Mussolini (1940); Wolin (1960, pp. 203–06); Namer (1961, 1973); Peirone (1971).

As a consequence of our liberation from "imaginary principalities," the "effectual" ones have burst the bonds of virtue. Virtue is no longer the standard by which they are judged, found wanting, and so deprived of our full allegiance. For so long as we think that we see in the nature of things a tension between what virtue and success or survival require of us, our devotion to virtue will not be wholehearted—but neither will our devotion to success or survival. We do not love that to which we stoop. Machiavelli cuts the Gordian knot: in redefining virtue as that which serves political success, he strives to abolish for all time to come the bad conscience[12] of the politically successful. The only virtue known to Aristotle that Machiavelli praises unequivocally is prudence.[13] It is not, however, Aristotelian prudence. Machiavelli deprives it of its intrinsic directedness towards virtuous ends, i.e., ends transcending mere political success.[14] It too now pursues "security and well-being" which the ancients had regarded as merely the conditions of virtue. Since virtue itself, however, is now a means to these, they need no longer be viewed as its conditions, or as conditions at all. There is nothing on which they are conditional. It is virtue which has become conditional, and no longer to be prized for its "imagined" intrinsic excellence. Henceforth Machiavelli will ignore what at the beginning of this chapter he had seemed to endorse: the presumed inherent superiority of what was formerly thought virtue to what was formerly thought vice. This is the key to the chapters that follow, in each of which Machiavelli treats of an erstwhile virtue. These he appraises from a new and wholly political perspective, that of the capable and ambitious individual who need no longer distinguish between what tastes good and what is good for him.[15]

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[10] Although this had not been true universally, it had been true of the greatest ancient thinkers as well as of a tradition in Christendom of which Machiavelli would have been aware, as also of Averroism. That the ancient thinkers had not exulted in this truth, as dangerous to the practice of virtue even in the most stable situations, explains their relative reticence concerning it. See the references in n. 14 below; also Plato, *Republic* 372a–374a, 422a–423b; *Laws* 709d–712a. On Christian antecedents and elaborations, respectively, of *raison d'état*, see Post (1964) and Mosse (1957).

[11] Against the notion that Machiavelli viewed the public good as in any way a limitation on the pursuit of private interest, see Spirito (1945, pp. 44–46); Scaglione (1956, p. 247); Strauss (1958, pp. 265–82); Ingersoll (1968, pp. 595–96); Anglo (1969, pp. 211–14, although he subsequently blurs the issue); Hexter (1957, 1964 [both 1973]); Rousseau (1973, pp. 58–61); Manent (1977).

[12] Maritain (1942, pp. 1–4); the rest of his presentation of Machiavelli does not live up to the boldness of this characterization.

[13] Prudence is absent from the list of qualities formerly considered to be virtues or vices; at the end of this list it emerges, however, as the touchstone for choosing among them. Prudence alone is always a virtue in a prince or the whole of the virtue of a prince.

[14] Aristotle, *Nicomachean Ethics* VI. Nourrisson's (1875, p. 289) remains the proper rejoinder to those who would make too much of Machiavelli's failure to counsel the prince to behave "badly" except where necessary: "In other words, it is as useful to the prince to stick with the good when there is no reason not to, as to know how to deviate from it when the occasion demands."

[15] On the character and thoroughness of Machiavelli's rupture with both antiquity and Christianity, and therefore with the thought of his day, see Chérel (1930, pp. 17–29); F. Gilbert (1939, not 1965); Strauss (1958); Flanagan (1972, pp. 142–46); Rousseau (1973, pp. 9, 38–42, 54–61); Manent (1977); Hexter (1973, p. 192): "The relation of the *Prince* to

II

The title of the first of these chapters is "Of Liberality and Frugality." Liberality has figured in chapter 15 in the catalogue of qualities for which princes are praised or blamed. There Machiavelli has called attention to the fact that it is a quality to which there are two unpopular alternatives, one of them consisting in forbearing to give and the other in not forbearing to take. He thus reminds us, at the beginning of a catalogue which thereafter lists only pairs, of the old-fashioned teaching of Aristotle concerning the qualities for which people are praised or blamed.[16]

Aristotle had depicted almost every virtue as a mean between extremes, the one deficient and the other excessive.[17] By virtue in general he had meant the sum of the particular kinds of excellence of which human beings are capable, which tend to a life both contented and worthy because finding its contentment in what is worthy.[18] Liberality is the particular virtue having to do with the use of property. Virtue with regard to wealth consists of superiority to wealth except as an instrument of virtue. It is a necessary instrument, for the fullest exercise of virtue requires self-sufficiency and the freedom and leisure which it confers. Seeing wealth nonetheless as merely the wherewithal for virtue, the virtuous man will rest indifferent to wealth exceeding the needs of virtue. He gets according to a standard other than the goodness of getting: the standard for economic activity is not itself an economic one. To give what one needs for oneself is extravagance; not to give what one does not need, stinginess. Liberality is the (nonarithmetical) mean between extravagance and stinginess.[19]

Of the political implications of liberality Aristotle says little. Liberality supposes means and hence a favored place in a stable political order. Aristotle does not discuss it as a way of climbing in the political order. Practiced for an ulterior motive, it would no longer be the virtue of liberality.[20]

Aristotle does hint, however, at a certain tension between liberality and the primary political virtue of justice. The problem arises from an ambiguity concerning the question of a surplus. Indifferent to a surplus as the liberal man may be in one sense, a surplus is itself prerequisite to the fullest practice of the virtue which manifests indifference to a surplus. The virtue of liberality consists in a superiority to surplus wealth which necessarily presupposes the possession of it. It therefore may be said to have the effect of expanding one's needs. The liberal man neither parts with what he needs nor fails to part with what he does not: he needs more than he otherwise would so that he may have something with which to part.

What is more, it is in the uncrabbed nature of the liberal man to tend rather to extravagance than meanness (IV, i, 18); Aristotle suggests more than once the difficulty of distinguishing between the virtue and its excess (II, viii, 5; IV, i, 31–32, 44). Liberality may be practically regarded as a virtue which consumes the basis of its practice. A too-liberal man will eventually confront the necessity of receiving from questionable sources so that he may continue giving so that he may demonstrate his indifference to receiving. Aristotle insists that in fact the liberal man will refrain from taking from improper sources. The reason that he alleges, however—that such a man is indifferent to wealth (IV, i, 15)—seems not to settle the question but to beg it. Aristotle had in fact begun by conceding that liberality, as opposed to justice, is a virtue which has primarily to do with how we use money rather than with how we acquire it (IV, i, 6–10).[21] He ends by redefining it so as to lend equal weight to the manner of getting (IV, i, 24), or by incorporating justice into liberality so as to resolve the tension between them. We come to see that the very distinction between liberality and the most outrageous extravagance—freely dispensing the goods of others—depends upon the primacy of justice, upon the recognition of what is justly one's own as the proper limit on one's giving to others. This makes a good deal of sense—so long as the motive of our liberality is the practice of the virtues generally, not for the sake of the

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the fabric of political imperatives of Machiavelli's own day should be evident enough: it makes a shambles of it." Cf. Singleton (1953).

[16] Which is precisely how Aristotle had described the virtues and vices, *Nic. Ethics,* II, v, 3. References in the text will be to these *Ethics.*

[17] *Nic. Ethics,* II, vi, 4–20; vii, 4; *Eudemian Ethics,* II, iii, 1–4.

[18] *Nic. Ethics,* I; *Eud. Ethics,* I–II.

[19] *Nic. Ethics,* IV, i, 1–14; *Eud. Ethics,* III, iv, 107; Cicero, *Offices,* I, xiv; II, xv.

[20] *Nic. Ethics,* IV, i, 34–35; Cicero, *Offices,* I, xiv.

[21] Cf. IV, i, 23: if we are reluctant to ascribe liberality to tyrants it is because they have so much rather than because of how they have gotten it.

reputation for them but from recognition of their intrinsic nobility.[22]

Liberality, practiced as it usually is, threatens to finish, by way of extravagance, in either injustice or meanness. Machiavelli's treatment of liberality begins from this problem in the account of Aristotle. He first directs our attention to it in his catalogue of qualities in chapter 15. There he both recalls and calls into question Aristotle's depiction of liberality as a middle way. He presents us with a triad of qualities, presumably the mean (or virtue) and its two extremes. One of these extremes, however, is the quality not of extravagance but of avarice. He immediately proceeds, moreover, to a second and seemingly superfluous listing of a pseudo-virtue of his own invention which would seem to be identical with liberality. This is the trait of the "giver" (*donatore*), here opposed to that of the "grabber" (*rapace*), stinginess having now dropped from sight. Machiavelli thus not only replaces extravagance with avarice or rapacity, but seems to suggest that it is these rather than stinginess which comprise the alternative to liberality most worthy of our attention. It may be that he means merely that this is the more odious of these alternatives, the one for which princes are most vehemently blamed and from which they would do well to keep the furthest. Chapter 16 will tell.

Not surprisingly, chapter 16 represents a considerable "improvement" over Aristotle, which expresses itself as a burlesque of him. It exposes the elements of his discussion to the harsh glare of Machiavelli's new perspective. It innovates, as we have been led to expect, by reconsidering liberality from the standpoint of the prospective giver who, doubting whether it is more blessed to give than to receive, would like to know whether it is more advantageous.

The title of this chapter is, as we have noted, "Of Liberality and Frugality (*parsimonia*)." (Rapacity, it seems, is hardly worth mentioning.) This seems to hint at the substance of its argument. In chapter 15 the tightfisted alternative to liberality had been identified as stinginess. It now appears that Machiavelli intends a rehabilitation of it—which, as a miser might, he now calls thrift—and a proportionate deprecation of giving. This hint is not altogether misleading.

The argument of the chapter runs as follows. It would be good for a prince to be thought liberal, but liberality so employed as to gain you the name for it is harmful. If you practice it "virtuously" you will not gain general acknowledgement of your liberality, and so will not have escaped a reputation for miserliness. (Not everyone will agree, presumably, that he received his due from you; cf. chapter 3. And those who do, while they will not resent you, neither will they be very grateful—for after all you have but given them their due; cf. *Discourses* I, 16.) It is not enough to make others love you to give them what is, from the perspective of the strictest virtue, the amount that you should give and that they should receive. In order, then, Machiavelli continues, to obtain the name of liberality (and it is only the name which is useful to you), it is necessary to practice extravagance. To gain the reputation for what Aristotle had praised as a virtue, you must practice in fact what he blames as a vice. This is not, however (needless to say), Machiavelli's own objection to the quest of a reputation for liberality. It is rather that in pursuing it you will consume your substance in lavish display, and so will be compelled either to abandon your giving or to adopt odious policies in order to extort new means from others. You will finish in meanness, which renders you despicable, or in oppression, which renders you hateful. Both will render you vulnerable.

A prince, then, "unable to use this virtue of the liberal man in such a way that he will be known for it, without harm to himself," should settle from the beginning for a reputation for miserliness. With time he will even come to be considered liberal, because his thrift will enable him always to live within his ordinary revenues, even in wartime. Thus "he comes to practice liberality toward all those to whom he does not take, who are innumerable, and miserliness toward all those to whom he does not give [i.e., towards those who had reason to hope that he might give], and these are few." Machiavelli's new sober, rational liberality consists not in giving to others but merely in not taking from them.[23]

Can a prince truly gain a name for giving simply by refraining from taking? Machiavelli himself now suggests his doubts. He begins his next paragraph with the observation that "therefore" (i.e., as he has shown) a prince

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[22] Cf. the previous note and IV, i, 11: one's reputation for liberality would seem to depend primarily on giving much and hence might increase with one's extravagance.

[23] Thévenet (1922, pp. 23–28); Rathé (1968, p. 527, n. 1); Peirone (1971, p. 60); and doubtless many others make out the teaching of this chapter to be just this one of thrift.

"should think nothing of incurring the name of miser, because it is one of those vices that enable him to rule." He thus admits that the practice of miserliness will incur the name—and therefore that miserliness is in fact, as here stated, a vice, because politically harmful. What follows confirms that although a reputation for miserliness may be less odious than one for rapacity, you become known for your liberality only by giving.[24]

Machiavelli proceeds to entertain two objections to what he has said thus far. The first is that many of the greatest princes have come to power through liberality. This Machiavelli concedes, only distinguishing between liberality as a way to power (of which he approves), and as its ornament, of which he does not. The second and more important objection is as follows:

> And if someone replies that there have been many princes who have done great things with their armies, and have been thought to be very liberal, I answer you: either a prince spends from what belongs to him and to his subjects, or from what belongs to others; in the first case, he should be frugal; in the other, he should in no way stint in his liberality. And to that prince who marches with his armies, who feeds himself on plunder, sacking, and ransoms, who disposes of what belongs to others, this liberality is indispensable.... And ... spending what belongs to others will not deprive you of your reputation, but will add to it; it is only spending your own which will hurt you.

Here we have a beautiful example of the scorpion's sting characteristic of Machiavelli's arguments. Although he concludes the present chapter with a reiteration of his counsel of thrift, and so with a show of respectability, the damage has been done. From an argument against concerning yourself with a reputation for liberality on the grounds that it will lead you into rapacity, Machiavelli has moved to an argument for rapacity—but foreign rather than domestic—as the means of procuring a reputation for liberality.[25]

What you give, you must have gotten. And if you wish to take, you must be prepared to give, for a readiness to satisfy the wants of others is essential to procuring their cooperation in satisfying yours. Liberality cannot be the basis of ruling because it depends itself on rapacity and indeed is merely a variation on rapacity: buying through giving, the robber chief dispensing the loot. As a clearsighted giver who recognizes that you give in order to be liked and that the giver is liked because the getter likes to get, you will see the necessity of yourself becoming a getter who does not depend on anyone's giving.[26] To raise the banner of liberality is to commit yourself to giving people what they want, as much and as often as possible, and what this implies or presupposes is a policy of perpetual expansion. The obverse of this banner is the jolly roger.[27]

III

Chapter 17 would have us reflect "On Cruelty and Compassion [or Mercifulness];[28] and whether it is Better to be Loved than Feared, or the Contrary." Passing from liberality to *pietà*, we pass from Aristotle to Christianity. Aristotle, and the ancient moralists generally, had placed little stock in solicitude for the sufferings of others, while of course reproving unjust harshness towards them.[29] The Fathers of the Church, however, elaborating the indications of Scripture, had made of love or charity[30] a cardinal virtue and the supreme rule of all human intercourse. And this love or charity issued above all in acts of mercy or compassion.[31]

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[24] Compare *Nic. Ethics*, IV, i, 10.

[25] Compare Cicero, *Offices* I, 14 on what passes in politics for liberality.

[26] Compare Strauss (1958, pp. 237–44).

[27] For comparisons of the teachings of chapters 16 and 17 with those of other Renaissance writers on their respective subjects see A. Gilbert (1968)—who, unfortunately, tends to miss the forest for the trees—and F. Gilbert (1939).

[28] Latin *pietas* (the chapter titles are in Latin); in the text, Italian *pietà*.

[29] The ancient thinkers present pity as a passion, and virtue as mastery of one's passions. Aristotle discusses pity not in his *Ethics* but in his *Rhetoric* (1385b–1386b); not in his account of those things for which people should be admired but in his manual of the passions by which they may be swayed. Consider also Plato, *Apology of Socrates* 35c; *Republic* 387b–388e, 602c; Seneca, *On Clemency* II, 5; Aulus Gellius, *Attic Nights* XIII, 16 (cf. Plato, *Euthyphro* 3d, which upon reflection confirms the claim of Aulus). Cf. Cope (1872, p. 59); Temple (1916, pp. 89–91); Livingstone (1952, pp. 53–59); Brumbaugh (1962, pp. 214–16); Vlastos (1971, pp. 16–17).

[30] Greek *agape*, Latin *caritas:* in each tongue one word which translators render variously as "love" or "charity." Recent discussions of this notion include Nygren (1974); Outka (1972). I am grateful for these references to Professor Bernard Zylstra of the Institute for Christian Studies, Toronto; errors in the discussion that follows are entirely my own.

Pietà, moreover, embraces "piety" as well as "pity." This ambiguity is no accident. A principal image of Christian piety, with double reason called *pietà*, juxtaposes the sufferings of God, by which He has redeemed men from endless suffering, with the pity which these evoke in the pious and which the image depicts in order to solicit. The Christian's solicitude for his fellow man follows from God's solicitude for the Christian and from the Christian's love of God. We are to love one another as He has loved us and because of our love of Him.[32]

Since, moreover, it is suffering which moves us to pity, our pity will vary with our perception of suffering. Christian *pietà* depends upon the Christian understanding of things. The suffering that stirs it most is the "imaginary" one of damnation.[33] Although it embraces the privations of the flesh as well, it also regards them as salutary in its primary concern with the soul. In bestowing its concern ultimately upon the salvation of souls, it differs radically from modern "humanitarianism."[34]

As earlier Machiavelli has ignored the "imagined" intrinsic dignity of classical virtue, so his treatment of *pietà* depends upon his dismissal of the cities of heaven and of God on earth, the "imaginary principalities" of Christian souls. He is the philosophic founder of pity liberated from piety, the first great "secularizer" of Christian compassion.[35]

Turning, then, to the chapter on compassion, we find that it begins as follows:

> Moving on to the other qualities previously mentioned, I say that every prince should want to be thought compassionate (*pietoso*) and not cruel; nonetheless he should be careful not to misuse this compassion.

Machiavelli implies that, differently from liberality, it is the practice of compassion which earns the reputation for it. A reputation for compassion being advantageous, so is its practice, subject only to certain qualifications.

> Cesare Borgia was thought to be cruel; nonetheless his cruelty brought order to the Romagna, united it, reduced it to peace and loyalty. He who thinks things through will see that [Cesare] was far more compassionate than the people of Florence, who in order to avoid a reputation for cruelty, abandoned Pistoia to its destruction.

Cesare was *thought* to be cruel; Machiavelli argues that he was not. It was Cesare who had been truly compassionate, the Florentines who had been truly cruel. Machiavelli uncovers a disparity between the "effectual" practice of these qualities and the enjoyment of the respective reputations for them. It is not merely that the practice of each quality may not gain you credit for it, but that the qualities themselves are the opposites of what most people conceive them to be, on the basis of which conceptions they grant reputations for them. Machiavelli moves from suggesting that we must sometimes be cruel rather than compassionate to suggesting that to be cruel *is* to be compassionate. In this chapter on compassion and cruelty the only example of compassion that he gives is one of cruelty—that of Cesare—and the only ones of effectual cruelty, those of the seeming compassion of the Florentines and of Scipio. The present paragraph concludes with a proclamation of the necessity of cruelty for all new princes. What seems to have sustained the argument thus far is moral indignation on behalf of a public-spirited cruelty which is willing to brave an odious reputation for the sake of the public good.[36]

Having opened the case for cruelty, Machiavelli presses it. Resorting, as in the previous chapter, to a parody of Aristotle, he presents the proper disposition in these matters—the virtue—as a mean between two extremes. These extremes are not, however, cruelty and compassion, but rather insufficient cruelty and excessive or gratuitous cruelty. The mean between the two is cruelty.

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[31] Matt. 6:12–15, 9:13; 22:37–40; Rom. 13:8–10; 1 Cor. 13; Gal. 5:13–14; Col. 3:14, 1 Tim. 1:5; 1 Pet. 3:8, 4:8; 1 John 2:8–11, 3:10–11; 2 John 1:5; Augustine, *Enchiridion on Faith, Hope, and Love [Charity]*, 31–32; *City of God* XXI, 22; Calvin, *Institutes of the Christian Religion*, II, 28, 51.

[32] Matt. 6:48, 16:24, 18:5 and 21–35; 25:34–46; Mark 12:28–33; Luke 6:36; John 13:34, 14:21, 15:9–12; Rom. 5:8; 2 Cor. 1:3–6, 8:7–10; Eph. 4:32, 5:2; Phil. 2:1–9; 1 John 4:7–21, 5:1–3; Augustine, *City of God* XXI, 26–27; Thomas à Kempis, *Imitation of Christ*, I, 15; Calvin, *Institutes* II, 28, 51; III, 7, 3–6; 8,1. On the impossibility of separating "Christian morality" from Christian belief, see also the sources cited in n. 37, especially Outka (1972, pp. 44–54, 181–206).

[33] Matt. 9:1–8, 18:7–9; Mark 16:15–16; Luke 16:19–31; 2 Cor. 1:3–7; James 5:19; Jude 1:17–23: Rev.; Augustine, *City of God* XXI, 16–18, 22, 24–26.

[34] For a contemporary illustration of this point, see Gray (1971, pp. 286–88).

[35] Cf. Chérel (1935, p. 24).

[36] Consider moreover the context (to which Machiavelli directs us) of the quotation from Virgil "in the mouth of Dido" (*Aeneid* II, 563–64): an assertion of the necessity of cruelty spoken in the midst of an act of mercy which will prove the ruin of both herself and her people.

Machiavelli, to do him justice, does not praise cruelty indiscriminately. As is explicit in his schema of the virtue, he does deprecate some cruelty as vicious, namely when it is too much or not enough cruelty. The cruelty that he recommends is cruelty "tempered by prudence and humanity." Prudence we know from chapter 15 as the "virtue" of self-serving calculation; for cruelty to be commendable it must be cold-blooded. As for humanity (*umanità*), it seems akin to mercifulness or compassion (*pietà*), while differing from it in two related respects. It lacks the Christian connotations of *pietà; umanità* is this-worldly or "humanistic." And it is differently related to cruelty. It is not the alternative to cruelty, but an aspect of cruelty properly practiced. There should be nothing surprising or confusing in this. For we have learned already that if by humanity we mean kindness toward other human beings, understood as merely human beings, the only "effectual" humanity is cruelty.

All of the preceding, however, poses a great problem for the ruler, which explains why this chapter must continue. "From this a controversy arises, whether it is better to be loved than feared, or the opposite."

Why does such a controversy arise "from this?" Because of a problem with Machiavelli's argument to which he himself has called our attention. His discussion began with the flat assertion that every prince should wish to be thought compassionate and not cruel, and this seems tantamount to saying that he should wish to be loved and not feared. In the meantime, however, cruelty has emerged as necessary to the public good. The prince thus faces a harsh dilemma. He should like very much to be thought compassionate rather than cruel, but the interests of his subjects (unbeknownst to the subjects) demand a statesmanlike severity. Good statesmanship, as we have learned, obtains the reputation for bad, and *vice versa;* life is unfair to good princes. Therefore this question of love and fear, compassion and cruelty, has not yet been resolved. As Machiavelli suggests here, it has merely been posed. "I answer that one would like to be the one and the other, but that since it is difficult to combine them, it is much safer to be feared than loved, should one have to do without one of them."

Machiavelli proceeds to make explicit the connection between compassion and love and between cruelty and fear. To be compassionate means to care, and you show that you care by wanting to help. Compassion manifests itself in politics, therefore, as the apparently unselfish satisfaction of the selfish wants of others. Professions of compassion on the part of political leaders have thus always proved inseparable from promises, express or implied, to indulge to the limits of their abilities the wishes of those for whom they profess to feel. Political compassion is at the same time flattery and attempted bribery.

In politics, then—that is, whenever you must deal with people as they are "generally"—you will become beloved by "serv[ing] the welfare" of your constituents as they perceive it. This leaves you, however, subject to them rather than they to you. Their love for you holds no solution to the problem of ruling, which is to obtain the cooperation of others in what is pleasing to you, whether or not it is pleasing to them and whether or not they wish to please you. Where their assistance depends upon a desire to please you which depends in turn upon your ability to please them, it dies with this latter, and hence when you most need it. Machiavelli's critique of compassion is a critique of a policy of trying to keep others in line by being nice to them. Such a policy presumes upon a reciprocity in human affairs and a justice in human hearts for which one "generally" looks in vain. It supposes political gratitude to rest upon an eagerness to repay past benefits rather than what is in truth its basis, the continuing expectation of future ones. It depends on one of two equally foolish assumptions: that you can call in your debts or that you will never have to. It therefore requires a supplement. This supplement is fear.

Machiavelli's argument, which we find only slightly altered in Hobbes, is that the fears of human beings are more to be relied upon than their desires. Interests shift, and it is beyond human power to govern their shiftings. Fear, however, subsists as a constant beneath the kaleidoscopic swarm of the appetites. If you command others' fear you may dispense with their love, for however far from you their desires may stray, fear of you will call them home again. Fear is the most reliable basis for the habit of obedience requisite to the success of a regime.

> The prince should nonetheless make himself feared in such a way that if he does not win love, he will avoid hatred; for to be feared and not hated can go very well together; and this he will always do, so long as he abstains from the

> property of his citizens and subjects, and from their women.

To be feared now appears as a kind of mean, one between being loved and being hated. To take is no less foolish than to depend on giving, for hatred dulls the edge of fear. Machiavelli's subsequent cruel remark on the relative depth of men's attachments to their fathers and to their patrimonies conveys at least this truth, that the more people own (and we own our patrimonies more fully than we own our fathers), the more for which they have to fear. For this reason alone the prince should leave them something to lose and should not be behindhand, prudence permitting, in giving them such. The present chapter modifies, but it does not overturn, the teaching of the chapter on liberality. The possessions of your subjects are useful to you, however, only so long as there is reason for your subjects both to fear for them and to fear displeasing you, while trusting in abstaining from so doing. Your behavior, therefore, should not be arbitrary. You should not take from those who keep within the bounds that you prescribe for them.

The maxim that a prince should be feared but not hated does not exhaust the teaching of this chapter. For it will be recalled that Machiavelli's answer to the question of love and fear is that a prince should wish to command both of them, and on this he has not reneged. He has, it is true, conceded the difficulty of combining the two. This does not, however, on the face of things, look to be impossible. You get to be loved by giving, and hated by taking; to be feared, which does not require taking, is then perhaps compatible with giving.

How being feared is compatible with giving is something that we learn from pursuing a reference. "He who thinks things through," Machiavelli has told us, will discern in the cruelty of Cesare Borgia his compassion. He thus directs us to chapter 7, in which Cesare's cruelty which "brought order to the Romagna" receives its careful examination. There we learn that having found this province in a state of the utmost lawless wretchedness, Cesare had speedily reduced it to peace and unity by means of a "cruel and able" deputy. Judging thereafter that such heavy-handedness might have rendered him odious, Cesare established a model government resting not on force but on law.

> And because he knew that past rigors had generated a certain hatred towards him, in order to purge the minds of those people and to win them over to himself completely, he wished to show that if there had been any cruelty, it had originated not with him but in the bitter nature of his minister. And having seized an opportunity to do so, at Cesena one morning he had him placed in two pieces in the public square, with a piece of wood and a bloody knife beside him. The ferocity of such a spectacle caused those people to remain at once satisfied and stupefied.[37]

Cesare was cruel, how much crueler than his deputy the latter learned too late. Yet his cruelty satisfied the Romagnuols as well as stupefying them, and if stupefaction is the height of fear, then satisfaction is just what Machiavelli means by love. Had Cesare allowed his minister to live, his repudiation of him would have lacked needed force. And he could not have dispensed with appointing another "cruel and able" deputy had he failed to establish the sufficiency of his personal reserves of cruelty. Cesare had then by the selfsame stroke to confirm his reputation for cruelty and to dispel the odium that this reputation had brought him. A lesser man might have failed to see his way here.

The smashing success of the crime of Cesare confirms a kind of cruelty as the compassion of princes. It further shows how this sort of kindness may find recognition as such. No ruler need choose in the end between the love and the fear of his subjects. By punishing criminals severely he may practice toward the innocent a compassion which both satisfies and stupefies, and which confirms them in their innocence while vindicating his own. As the enemy of both the prince and his subjects, the criminal class forges the bond between them; by showing himself its implacable enemy the prince may continually renew that bond. By punishing those subject to him who prey on the others, the ruler obscures what he has in common with them. Plunder, when employed against those who plunder—a use of it fully in the prince's interest—will no longer be viewed as plunder, but as taxes.[38]

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[37] For a variant version of the text of this passage which does not however affect our interpretation, see Whitfield (1967, p. 16).

[38] Many—such as Janni (1930, p. 282); Alderisio (1930, pp. 176–78); Bizzarri (1940, p. 105); Wolin (1960, pp. 220–24); Namer (1961, pp. 144–50)—have sought to moralize the immoralism of Machiavelli by urging his preoccupation with "the economy of violence." Machiavelli's concern, however, is in fact the efficacy of violence, the foremost criterion for which is that it meet with no effective violence in return. Machiavelli's counsel of "violence" does not grow from squeamishness about "violence." It expresses disgust with squeamish violence as half-hearted and ineffective violence.

Leniency towards the guilty, conversely, is harshness towards the innocent such as might encourage them to emulate the guilty. In addition it implicates the ruler in the crimes of the guilty and earns him some of the resentment due them. By appearing indifferent to the wrongs of his loyal subjects, he will gain a reputation for cruelty among them.[39]

IV

Machiavelli's approach to virtue is a frankly utilitarian one: virtue is as virtue gets. Such an approach necessarily implies permission to those who follow to tinker. Liberal political thought has consisted in great part in tinkering with Machiavelli and with those who have tinkered with him.

For Machiavelli, as we have seen, liberality and mercifulness are not the ends which peace and prosperity (however purchased) are good because they further. Instead these "qualities" are themselves good only because—and therefore only inasmuch as—they further peace and prosperity. Thinking through the beneficent virtues without regard for "imaginary" limits, he liberates the practice of these virtues from all but practical restrictions. Liberality "practiced as one ought to practice it" gives way to liberality as means to political success. Giving is politically advantageous, but only because receiving is pleasant. Because there are with few exceptions no limits to the human thirst for receiving, there can be none on the propriety of giving. Since giving, however, depends upon taking, the prince must take from others to give to his own. The prince then should place himself at the head of his people, as general of an army of takers. He satisfies by unleashing acquisitiveness in conditions conducive to its satisfaction.[40]

Machiavelli's warlikeness is perhaps the chief thing that seems to separate him from his successors and from us. It is what sometimes leads him to seem to stigmatize personal greed (*Prince*, Ch. 12; *Discourses* I, 37; III, 25) and has gained him the reputation of one who was inimical to acquisition. But Machiavelli's warlikeness may depend upon his conviction of the inevitability of war and the consequent necessity of pre-emptive expansion.[41] In chapter 16, in any case, he preaches war, but war specifically as a means to gain. His successors brought not a sword but peace, but with it no sacrifice of the means to gain. Labor, investment, trade, and science—all of which cry out for peace—supplant war as the instrument of avarice (and even of glory).[42] The improvement over Machiavelli is technical. The collective struggle for acquisition may proceed on a permanent and general footing, the victories coming bloodlessly at the expense of nature rather than of some other man or nation. The old warlikeness is obsolete, and so too is its attendant austerity. Property is no longer static, so there may be opportunity for all of a sort that blunts the edge of class warfare (cf. *Prince*, Ch. 9; *Discourses* I, 37, 55). We can create wealth rather than confiscate it. We do so not to limit acquisition, but so that there may be acquisition without limit.

As liberalism follows Machiavelli in denying that there are in principle limits to collective acquisitiveness, so it also accepts his transformation of the older notion of *pietà*. From the first its compassion is this-wordly, unconcerned with the imagined pains of imagined other worlds, unwilling to accept imagined bliss as compensation for the pains of this one. It accepts also that as the truest liberality is to free human beings to indulge their inclinations at the expense of nature ("externally"), so the truest compassion is to protect them from those who would indulge such inclinations at the expense of others ("internally"). In agreeing that our natural interest lies in doing what we wish in safety, we limit the mission of politics to guaranteeing the requisite safety.[43] The most effective compassion is preventive: rather than waiting to succor misery, it spares us pain through the judicious application of it. It becomes the cruelty that saves from cruelty, helping us by hurting those who would hurt us. What both satisfies and stupefies is law and order.

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[39] Such, for instance, was the fate of Scipio in the eyes of the people of Capua, as described at the conclusion of our chapter.

[40] The economic theory with which Machiavelli broke has been described in a convenient epitome of it (Stark, 1956) as a theory of a "contained economy." A melange of ancient and Christian thought, all of its different representatives held to the primacy of virtue and the necessity of restraining acquisition accordingly. There is no adequate published treatment of the seminal economics of Aristotle (*Politics*, Bk. I); for introductions to medieval thought see Tazzi (1970), de Roover (1971), Capitani (1974). The most recent overview is Gordon (1975).

Historians of economics rarely mention Machiavelli, and still more rarely do they mention him as more than a forerunner of mercantilism. Consider, however, Spann (1930, p. 76): "That which seemed so uncongenial to persons still nourished on medieval traditions, the exclusion of all moral conceptions from the domain of political science (an exclusion which Machiavelli was the first to advocate) was erected by Quesnay with certain reserves, and by his [Physiocrat] disciples more radically, into a finished system."

[41] In that case his argument would differ but little from that of the peace-loving Hobbes concerning the exigencies of the state of nature, in which the only defense is a good offense (*Leviathan*, Ch. 13). For a "Hobbesian" interpretation of Machiavelli, see the most interesting piece by Manent (1977).

[42] Locke, *Two Treatises of Government*, II, v; Montesquieu, *Spirit of the Laws*, XX, XXI; Hume, "Of Commerce," "Of Luxury"; Adam Smith, *The Wealth of Nations* III, iv (ed. Cannan) (New York: Modern Library, 1937), p. 385. For citations in this vein from lesser writers, see Hirschman (1977).

The question of war apart, the difference between Machiavelli and his successors has to do with tyranny. Machiavelli cannot gainsay tyranny because he concedes the primacy in us of passions that are both selfish and practical (cf. chapters 3, 9, 17–19). His successors reject tyranny, but not because they reject this premise. They debunk the prince not for his selfishness but for his irrationality when viewed from the perspective of cautious selfishness. In the interests of political peace, they deprecate the egoism of glory in favor of that of safety and wealth.[44] The princely or ambitious character is reduced to a means to the welfare of the people as their agent or "representative."[45]

Liberalism agrees with Machiavelli in understanding virtue as a means to prosperity and security.[46] It does not understand the fostering of acquisitiveness primarily as manipulation of the people. It continues to understand it, however, as benefiting the people rather than corrupting it. In this the Marxist critics of liberalism disagree with it partly in overestimating the ultimate benefits to accrue from acquisition. All modern parties accept Machiavelli's moral revolution.

Machiavelli relieves us of our qualms; in chapter 16, over dangling the carrot; in chapter 17, over brandishing the stick. He thereby lays the moral foundation for societies based, as all modern societies have been, on collective aggrandizement recognizing no limits in principle, and on justice understood as "equal (but otherwise boundless) opportunity."

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[43] Hobbes, *De Cive,* V; *Leviathan,* Introduction; Chs. 14, 15, 17 and *passim;* Locke, *Treatises,* II, ix; Hume, *Treatise of Human Nature,* II, vii; Adam Smith, *Theory of Moral Sentiments,* II, i, 5 (n.); II, ii, 3.

[44] Hobbes, *De Cive,* I, 4, 12; *Leviathan,* Ch. 13; Locke, *Treatises,* II, v, 34 (the deprecation of the "quarrelsome and contentious" in favor of the "industrious and rational"); cf. Adam Smith's characterization of the feudal nobility, *Wealth of Nations,* III, iv, and Bacon's Machiavellian reconciliation of glory and pacificity, *The New Organon,* Aphorism 129.

[45] Representative government was conceived as an attempt to subjugate princely to popular interests through institutions which would simultaneously exploit ambition and hamper it; the classic statement is *Federalist* 10. As is much less widely appreciated, it was at the same time an attempt to reconcile peoples to princes (and hence to civic order) by rendering their subjection invisible. In this aspect it resembles religion as conceived by Machiavelli (cf. *The Prince,* chapters 6 and 11 and the crucial importance of "prophecy," which enables you to present yourself as a servant rather than as a ruler). On this see Mansfield (1971); on the Machiavellian roots, Mansfield (1970).

[46] Hobbes, *De Cive* III, 31–32; Leviathan, Chs. 14 and 15; Locke, *Treatises,* II, ii, 8 (on the purpose of the "Law of Nature"); Hume, *Treatise,* II, ii; III, vi; Tocqueville, *Democracy in America,* II, ii, 8; Nietzsche, *Genealogy of Morals,* I, 1–3.

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Power and Social Exchange*

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This article examines the basic social science concepts of "power" and "social exchange" in order to determine the possibility and desirability of integrating them. It is argued that: (1) all exchange relationships can be described in terms of conventional power concepts without twisting the common-sense notions that underlie such concepts; (2) most—but not necessarily all—power relationships can be described in terms of exchange terminology; (3) there are some advantages to conceiving of power in this way; (4) recent social exchange theorists have neither illuminated nor recognized most of these advantages. After a preliminary examination of the concepts of "power" and "exchange," the discussion focuses on the analytical and conceptual problems associated with volition, exchange media, asymmetry, sanctions, and authority.

Conflict studies, power analysis, and social exchange theory are three overlapping yet distinguishable bodies of social science literature. The purpose of this article is to examine some of the areas of overlap in order to determine whether conceptual integration is feasible and/or desirable. I will focus primarily on power and social exchange, but the implications for the study of conflict are significant. A recent study, for example, criticized conflict analysts for neglecting power and proceeded to combine conflict, power, and social exchange theory into a single model (Korpi, 1974). Also, since most power analyses treat conflict as a necessary condition of power (Nagel, 1975, p. 154), such studies may be viewed as a special type of conflict research.

Improved understanding of the feasibility and/or desirability of conceptually integrating power analysis and social exchange theory should aid in choosing overall social science research strategies. Dahrendorf (1958, p. 127) has argued that at least two basic social science models are needed because "society has two faces of equal reality: one of stability, harmony, and consensus and one of change, conflict, and constraint." Similarly, Eckstein (1973, p. 1161) argues that "there are two fundamental sciences of society: that dealing with symmetrical social relationships and that dealing with asymmetric ones in social units—'economics' and 'politics.'" There are two basic models of social interaction, however, that may *combine* cooperative and conflictual approaches.[1] Harsanyi (1969, p. 515) claims that modern game theory has demonstrated that "the same theoretical model can handle both conflict and cooperation without any difficulty." The question here is whether social exchange theory can also be used to explain both faces of social reality. The adequacy of such "explanations" has important implications for deciding how one stands on the questions raised by Eckstein and Dahrendorf.

How useful are exchange models in analyzing power relations? According to Homans (1974, p. v), the word "power" does not even appear in the original edition of his landmark study of social exchange, *Social Behavior: Its Elementary Forms* (1961). The title and the contents of Blau's *Exchange and Power in Social Life* (1964) clearly imply that exchange and power constitute two different realms of human relations. Curry and Wade (1968, p. 118) have noted that critics of exchange models often claim that power "cannot be conceived of in exchange terms by definition since no exchange occurs in a power relationship—there are only 'winners' (the powerful) and 'losers' (the powerless) and no two-way distribution of rewards and costs." The question of whether and how exchange models can be used to enhance our understanding of power has no generally agreed-upon answer.

It will be argued that: (1) all exchange relationships can be described in terms of conventional power concepts without twisting the common-sense notions that underlie such concepts; (2) most—but not necessarily all—

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[1] Roger Masters has called my attention to ethology as a possible third approach that treats both cooperation and conflict in social interaction (Masters, forthcoming).

power relationships can be described in terms of exchange terminology; (3) there are some advantages to conceiving of power in this way; (4) recent social exchange theorists have neither illuminated nor recognized most of these advantages. After a preliminary examination of the concepts of exchange and power, the discussion will focus on the analytical and conceptual problems associated with volition, exchange media, asymmetry, sanctions, and authority.

Exchange Models and Power Concepts

Robert Dahl has suggested that "power terms in modern social science refer to subsets of relations among social units such that the behaviors of one or more units . . . depend in some circumstances on the behavior of other units" (1968, p. 407). The intuitive idea or common-sense notion behind power terminology has been described by Dahl (1957) in terms of A getting B to do something he would not otherwise have done. Is this notion adequate to describe an exchange relationship?

Suppose A's goal is to get B to sell a loaf of bread. Walking into B's store, A has several options in choosing what kind of influence attempt to make. A can pull out a gun and say, "Sell me the bread or else." He can get down on his knees and beseech B to sell him the bread. He can offer B a million dollars if B will sell him the bread. Each of these strategies has some chance of success, but each also involves some costs. The best way for A to balance costs and benefits is probably going to entail checking the price tag and offering B the amount of money printed on the tag. A walks out of the store with the bread, having succeeded in getting B to do something that B would not have done in the absence of A's influence attempt. An influence attempt has succeeded; an exchange has occurred. Given Dahl's broad concept of power, exchange relations are simply subsets of power relations (Baldwin, 1971a, pp. 581–92).

Can power relations be described in terms of exchange concepts? When nation A gives nation B foreign aid in return for support in the United Nations, we may say that nation A has used foreign aid to influence nation B's behavior in the United Nations (assuming that the foreign aid actually made a difference in nation B's behavior). It would be just as easy, however, to describe nation A as having *exchanged* foreign aid for support in the United Nations. Thus, at least some power (or influence) relations take the form of exchange.

The difficulty arises when one introduces negative sanctions (threats and/or punishments) or environmental manipulation into the power situation. "Your money or your life" can be converted into exchange terminology as follows: "You give me your money, and I will let you keep your life." Some people, however, object to calling such a transaction an "exchange" (Blau, 1964, pp. 115–16; Boulding, 1963, 1965). The phrase "your money or your life" is usually attributed to an unsavory gun-wielding outlaw, which may account for some of its bad press. The phrase is rarely attributed to the physician who has just informed the patient that he will die within a year unless he can afford the expensive operation required to save his life. In such a situation, depicting "your money or your life" as a proposed exchange may not seem quite so objectionable.[2]

Environmental manipulation is another power situation that is difficult to describe as exchange. If A can secretly control the temperature in B's room, A can get B to take off a sweater without B ever knowing about A's influence attempt. Although Dahl's broad concept of power would consider this a power relationship, most people would be reluctant to call it an exchange relationship. There are, then, some power situations that are hard to describe with exchange concepts.

Although exchange relations can be considered a subset of power relations, the social exchange theorists have not provided a satisfactory account of how power relations work. Blau (1964, pp. 4, 6, 88–89) recognizes two concepts of social exchange, one broad, the other narrow. Although the broad one would subsume power, Blau rejects it, first, because he fears his theory will become tautological, and second, because "nothing is gained" by trying to force actions such as power relations into a conceptual framework of exchange.[3] Blau (1964, pp. 115–42) also recognizes two concepts of power, one broad enough to include exchange, the other narrow enough to exclude it.

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[2] Time may also be a critical factor in explaining people's reluctance to view "your money or your life" as an exchange. The typical mugging occurs so quickly that the "muggee" does not have time to adjust his or her value expectation baseline (see Baldwin, 1971b, p. 23; and Blau, 1964, p. 116) to the new situation. Thus, the mugger's offer of life does not seem especially generous. In airline hijackings or concentration camps, however, there is time for the new probability of losing one's life to be built into one's value expectation baseline. Once this occurs, "your money or your life" is more likely to be viewed as a genuine exchange. It is not uncommon to hear stories of victims thanking guards or hijackers for allowing them to keep their lives.

> Broadly defined, power refers to all kinds of influence between persons or groups, including those exercised in exchange transactions, where one induces others to accede to his wishes by rewarding them for doing so. . . . Although the customer technically imposes his will upon the jeweler when he makes him surrender a diamond ring by paying for it, this situation clearly should not be confused with that of the gangster who forces the jeweler to hand over the ring at the point of a gun (Blau, 1964, pp. 115–16).

Blau thus admits the "technical" possibility of depicting exchange as a power relationship but implies that doing so would confuse the distinction between customers and gangsters. It is quite possible, however, to use a broad concept of social power while preserving the distinction between influence attempts based on positive sanctions (actual or promised rewards) and influence attempts based on negative sanctions (actual or threatened punishments) (Dahl, 1968; Baldwin, 1971b). Blau opts for a narrow definition of power that completely rules out positive sanctions.[4] Thus, instead of synthesizing the concepts of exchange and power, Blau accentuates the cleavage between the two concepts.

In Homans' revised edition of *Social Behavior: Its Elementary Forms* (1974), he devotes a whole chapter to "power and authority" (pp. 70–93). For Homans, power relations are a subset of exchange relations in which one person "gets less" out of the exchange than the other.[5] Homans defines power broadly, however, so as to include both positive and negative sanctions. Thus threats and punishments can be exchanged either for rewards or for other threats and punishments. Homans (1974, pp. 79–81) analyzes the "your money or your life" situation as an exchange and brings out *both* the similarities and differences between coercive exchange and noncoercive exchange. Even though there are drawbacks to Homans' concept of power, which I shall address later, his broad concept of power and his attempt to integrate the concepts of power and exchange are steps toward a synthesis.

To summarize, if one uses the broad concept of power associated with Dahl (1968), exchange relations appear to be subsets of power relations. For Blau (1964), however, exchange and power are separate and distinct realms; neither is a subset of the other. For Homans (1974), power relations are subsets of exchange relations. Obviously, the exchange theorists use a narrower concept of power than Dahl does. It is interesting to note that neither Blau (1964) nor Homans (1974) cites Dahl (1957, 1968) or Lasswell and Kaplan (1950). Having introduced the general problem of integrating power concepts into exchange models, I shall next examine several special topics related to such an undertaking.

Volition

"Power," in Max Weber's classic definition (1947, p. 152), "is the probability that one actor within a social relationship will be in a position to carry out his own will despite resistance, regardless of the basis on which this probability rests." The words "despite resistance" are often interpreted as precluding depiction of power as a kind of exchange. Exchange, it is argued, is voluntary, while power involves A getting B to act "against his [B's] will." Thus, power relations are characterized by conflict, while exchange relations are characterized by cooperation.

Three questions may be asked about dichotomizing power and conflict on the one hand and exchange and cooperation on the other: (1) Is it useful for social analysis? (2) What does it mean? (3) Is conflict present in a routine commercial transaction? If one conceives of most social situations as mixtures of conflict and cooperation, with pure conflict and pure cooperation as polar types rarely found in the real world, then the conflict/cooperation dichotomy is not likely to appeal. Harsanyi (1969, p. 515), Schelling (1960), and Masters (forthcoming) have argued in favor of models that incorporate both conflictual and cooperative elements. Power without cooperation and exchange without conflict are not prototypical cases, but rather bizarre extremes rarely encountered in real life. Schelling (1960) has shown that even war, often thought to be an example of pure conflict, has significant cooperative dimensions.

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[3] Blau simply asserts that "nothing is gained"; he does not prove it. Likewise, March (1966, p. 65–67) acknowledges the possibility of using exchange models to explain power but asserts that we probably would not want to do so.

[4] Chadwick-Jones (1976, pp. 281, 294, 299–300) claims that "reward power" is Blau's main concern. This is a questionable interpretation of Blau, however, since Blau (1964, p. 117) explicitly defines power in terms of negative sanctions and even says that "inducing a person to render a service by rewarding him for doing so does not involve exercising power over him. . ." (p. 141). Exchange (based on positive sanctions) and power (based on negative sanctions) are separate and distinct realms for Blau (see Baldwin, 1971b).

[5] We shall take up the question of what it means to "get less" from the exchange at a later point.

We might also ask what it means to describe B as acting involuntarily or "against his will." If one were to ask whether someone would like to work eight hours a day on an assembly line, it is doubtful that any answer whatsoever would be forthcoming until the reward (wage) was specified or implied. I know very few college professors who would say "yes" if the wages were three dollars per hour, but I know very few who would say "no" if the wages were a million dollars per hour. The point is, of course, that B can make no meaningful assessment of his "will" without taking opportunity costs into account—and neither can the social scientist who wants to explain B's response to A's influence attempt. To describe a power relationship in terms of A getting B to do something against his will is to obscure the heart of the power process, i.e., A's manipulation of the incentives (or opportunity costs) that B associates with various courses of action (Harsanyi, 1962). It is precisely B's "will" that A is trying to change. The apothegm that "every man has his price" is no doubt false, but it represents an analytical perspective that is more useful to the student of power than the perspective that depicts people as "acting against their wills" (cf. Oppenheim, 1961; Blau, 1964, pp. 91–92).

This is not to say that measuring (or estimating) B's willingness to do X (perceived opportunity costs of doing X) is not important in measuring A's power over B; the point is that B's willingness to do X should not be dichotomized so that B is either "willing" or "unwilling" to do X. Parsons and Smelser (1956, pp. 10, 13) have pointed out that the same logic that underlies the economists' supply and demand schedules also "applies to the performance-sanction relationship in all social interaction." In other words, the likelihood that B will perform X may depend on the sanction (positive or negative) provided by A. Supply and demand schedules may thus be considered as describing a kind of power relationship. To ask whether B is "willing" to do X is like asking whether General Motors is "willing" to provide you with a car. Neither question can be answered satisfactorily until the price is specified or implied.

Some would deny that a routine commercial exchange involves a significant amount of conflict. The mutual interest of both buyer and seller in reaching agreement and the mechanics of the price system tend to obscure the conflictual aspects of the situation. From the standpoint of the buyer, the lower the price the better. From the standpoint of the seller, the higher the price the better. The exchange will be consummated only if the highest price the buyer is willing to pay overlaps with the lowest price the seller is willing to accept. The price system enables both A and B to cut the costs of acquiring information about each other's bargaining position. The establishment and promulgation of a "going market price" for a loaf of bread provides each with information about what price is likely to be acceptable to the other. Thus, the observer of a routine purchase of a loaf of bread is likely to see very little evidence of conflict, but this is primarily because the price system facilitates rapid resolution of the conflict and agreement on a "fair price."[6] If the same observer were to witness a routine economic exchange in a nonmarket economy, the evidence of conflict between buyer and seller would be more obvious.

In sum, the problem of volition does not provide an insuperable obstacle to conceiving of power as exchange. Both conflict and cooperation are present in most social situations, and the willingness of B to comply with A's demands is a function of the opportunity costs that B associates with compliance. Since the purpose of A's influence attempt is to change B's perceived opportunity costs of compliance (and/or noncompliance), it is not very helpful to describe A as influencing B by getting B to do something "against his will" or "despite his resistance."

Media of Exchange

Some see power as distinct from exchange (Blau, 1964); some view power as a kind of exchange (Homans, 1974); and others treat power as a medium of exchange (Parsons, 1963; Deutsch, 1963). Many questions are raised by such viewpoints, but only two will be addressed here. First, what are the implications of considering power as a medium of exchange akin to money? And second, how is economic exchange different from other kinds of exchange? Since I have presented an extended critique of the "power as money" analogy elsewhere (Baldwin, 1971a), only brief recapitulation is needed here. The most important point to be made is that the standard social science concept of power[7] as a *relation* is incompatible with the "power as money" analogy. Although purchasing power may be conceived of as a kind of power relation, money is more like a power resource or power base. If one thinks it useful to distinguish between power resources and power relations, one should be wary of attempts to depict power as a medium of exchange. Parsons (1963, p. 232) has explicitly complained that Dahl's concept of power makes it logically impossible to treat power as a "mechanism operating to bring about changes in the action of other units." This is true, since Dahl's concept of power refers to a relationship rather than a mechanism. Power defined as a relation refers to the *process* of A getting B to do something B would not otherwise do; but power defined as a medium of exchange refers to one of several *means* by which A can affect B's behavior (Baldwin, 1971a).

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[6] For an expanded discussion of the role of conflict in economic exchange that also notes the tendency of the market to disguise the dynamics of the situation, see Boulding (1965). Since the market facilitates "anticipated reactions," it is difficult to observe influence relations in routine commercial transactions. Nagel (1975, p. 145) handles this analytical problem very well by pointing out that "in studying influence, attention should not generally focus upon exact sequences of action and response." Such interactions, he notes, are often present but are not a prerequisite for influence to exist.

Much of the confusion about treating approval, status, esteem, compliance, reputation, or power as media of exchange (Waldman, 1972; Blau, 1964; Homans, 1961, 1974; Chadwick-Jones, 1976) could be eliminated by clarifying the differences between economic exchange and social or political exchange. If one is referring to direct exchange (pure barter) without a common denominator of value,[8] there are not many differences among the following exchange situations:

One cow for two pigs
Two sacks of wheat for five days of work
One favor for another
Esteem for a favor
Love for love
Compliance with a request for approval
A vote in the United Nations for foreign aid.

In each of these situations the trading partners will have difficulty deciding whether they have made a "fair trade," since they have no standardized measure of value for reference.[9]

Blau (1964, pp. 8, 93–95) distinguishes social exchange from economic exchange in terms of the specificity of the obligations incurred. When B does A a favor, A incurs an obligation to do B a favor sometime in the future. A's feeling of indebtedness to B, however, lacks specificity because there is no generally recognized common denominator of value for comparing the worth of various favors. "In contrast to economic commodities, the benefits involved in social exchange do not have an exact price in terms of a single quantitative medium of exchange. . ." (Blau,

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[7] Reference to a standard or conventional social science concept of power may strike some readers as presumptuous, since it implies some consensus on power terminology. The frequently heard assertion that there is no such consensus is simply not true. Consensus is a matter of degree and need not imply unanimity. The consensus may be only partial but it certainly does exist. The significant thing is not that Dahl (1968) and Cartwright (1965) found it difficult to summarize and integrate the power literature but that they were able to do it so well. The works by Lasswell and Kaplan (1950), Dahl (1968), and Cartwright (1965) have many differences, but they have enough in common to justify reference to a "standard" or "conventional" approach to the study of power. For example, all three works conceive of influence as a relation rather than a property; all three stress the need to specify scope; and all three allow for both positive and negative sanctions in discussing influence. Future references to the conventional social science concept of power in this article refer to the general conceptual outlook of these three works. It should also be noted that for purposes of this article the terms "influence," "power," and "control" are used interchangeably. Furthermore, all references to A's effect on B's "behavior" in this article should be interpreted to include changes in B's beliefs, attitudes, opinions, expectation, and/or emotions as well as changes in observable behavior (cf. Nagel, 1975, p. 12).

[8] Technically, pure barter implies direct exchange, but need not imply the absence of a standardized measure of value. It is thus possible for the measure-of-value function of money to be performed by something other than that which performs the medium-of-exchange function of money. As a practical matter, however, the two functions are so highly interdependent that it is difficult to imagine pure barter in the presence of a common denominator of value (cf. Baldwin, 1971a; Blau, 1964, pp. 268–69). For purposes of this article, therefore, references to barter situations imply the absence of both exchange media and a standardized accounting unit.

[9] Strictly speaking, a standardized measure of value facilitates judgments as to the fairness of an exchange but is not a logical necessity. It is hypothetically possible to imagine a pure barter market in which every item traded had a "going" or conventional exchange rate in terms of every other item. If 1,000 items were traded in the market, each item would have 999 "going market prices." For markets above a certain size, money may be a practical necessity; but it is not a logical necessity.

1964, p. 94). It is clear that what Blau has in mind is *not* the general case of economic exchange but rather the special case in which a recognized standard of value is operating as a medium of exchange.

Since barter is so rarely encountered in everyday economic exchange, one might ask why it matters which concept of economic exchange Blau uses. The answer is that only by understanding precisely what difference money makes in economic exchange can one understand the role other media of exchange play in noneconomic exchange. The key to understanding social and economic exchange is recognizing that there is one—and only one—important difference between economic exchange in a monetary economy and social or political exchange. That difference is the presence of a generally recognized measure of value that also serves as a highly liquid medium of exchange, i.e., money. It is money that sets economic exchanges apart from other kinds of social interaction.

Blau (1964, pp. 93–95) claims that "social exchange differs in important ways from strictly economic exchange," but all the differences he identifies can be reduced to one—the absence of a counterpart for money. When Blau notes the unspecified obligations arising from social exchange, he is simply pointing out one of the implications of trade without money. When Blau cites the absence of an "exact price in terms of a single quantitative medium of exchange" as "another reason why social obligations are unspecific," the citation is both misleading and redundant. The absence of "an exact price in terms of a single quantitative medium of exchange" is not an additional reason why social obligations lack specificity; it is the *only* reason.

When Blau (1964, p. 170) portrays the "compliance of others" as a "generalized means of social exchange, similar to money in economic exchange (except that it is far less liquid than money)," he implies that money and compliance have a lot in common but differ in degree of liquidity. It is as if he had said, "John is similar to Bill except for his red hair." One would conclude from this that John and Bill share many characteristics other than hair color. The difficulty here is that liquidity is not just one of several characteristics of money; it is the essential defining characteristic. Blau (1964, p. 269) admits as much later in the book when he observes that "money differs from other valuables only in the higher degree of its liquidity, that is, the greater ease with which it can be converted into other commodities." We may now recast Blau's earlier comparison of compliance and money as follows: "Compliance of others is similar to money except with regard to the only characteristic that really matters, the degree of liquidity."

To summarize, the analogy between power and money is rejected as incompatible with the standard social science usage of the term "power." Economic exchange is not much different from social exchange except in a money economy. Although indirect exchange, i.e., exchange via media of exchange, is both conceptually possible and empirically probable in noneconomic exchange, no media bearing a significant resemblance to money are likely to be found. Instead of *comparing* the media of noneconomic exchange with money—as exchange theorists are prone to do (Chadwick-Jones, 1976, p. 20; Blau, 1964, p. 22; Waldman, 1972, p. 77)—one should emphasize the *contrast* between economic and noneconomic media of exchange. In order to examine the opportunities and limitations of exchange models in the study of power, one must recognize that money is a very unusual (one is tempted to say unique) medium of exchange that also serves as a standardized measure of value. In short, economic exchange (in a money economy) is different because money is special. Attempts to obscure the special qualities of money make it difficult to adapt exchange models to the study of power.

Asymmetry in Power Relations

The most important obstacle to analyzing power in terms of exchange is the concept of power as an asymmetrical human relationship. As noted earlier, Eckstein (1973, p. 1161) even suggests that the distinction between symmetric and asymmetric social relationships delineates "two fundamental sciences of society." Economics, based on exchange and symmetry, and politics, based on power and asymmetry, are thus relegated to fundamentally different categories of social interaction. Such a distinction discourages attempts to synthesize power analysis and exchange analysis.

Exchange theorists might be expected to oppose the conception of power as asymmetrical, emphasizing instead the reciprocal nature of power relations. Not so. Far from rejecting the asymmetrical concept of power, exchange theorists (Homans, 1974, pp. 70–71, 77, 83; Blau, 1964, pp. 117–18, 312–13; Chadwick-Jones, 1976, pp. 299–300, 355–57; and Ilchman, 1971, pp. 18–19) have embraced it and thereby compounded the difficulties of incorporating the standard social science concept of power into exchange models. These difficulties concern the ambiguity of the notion of asymmetrical power, the need to specify scope in defining power, and the treatment of costs.

At least four notions of "asymmetry" can be found in discussions of the inherently asymmetrical nature of power, including asymmetry of causation, imbalance of influence, unequal benefits, and uneven distribution of power. Power may be conceived of as a causal relationship, and all causal relationships are asymmetrical in the sense that if event C causes E, E does not cause C (cf. Oppenheim, 1961, p. 104; Dahl, 1968, p. 410; Nagel, 1975, pp. 35–51, 141–53; and Simon, 1957, pp. 5, 11–12, 66). Simon (1957) and March (1955) suggest that power is also asymmetrical in the sense that if A has power over B, B does not have power over A. March (1955, p. 436) claims that "the statement that A *influences* B excludes the possibility that B *influences* A."

There is a big difference, however, between saying that event E caused event C and saying that person A influenced person B. That difference is implied by the term "scope" (Lasswell and Kaplan, 1950, pp. 73, 77). Since people perform different activities, it is quite possible—and highly probable—that A's power over B will be limited to certain dimensions of B's behavior. Thus, person A may be influencing person B with respect to X at the same time that person B is influencing person A with respect to Y. It is for this reason that many power theorists consider a statement of an influence relationship that fails to specify scope as virtually meaningless (Dahl, 1976, p. 33; 1968, p. 408; Lasswell and Kaplan, 1950, p. 76; Nagel, 1975, p. 14).

It is not the purpose of this discussion to deny either the causal nature of power or the asymmetrical nature of causation. It is to point out that attempts to discuss the implications of causal asymmetry for power analysis often lead to confusion because of failure to give sufficient attention to the specification of scope. Causal asymmetry does not preclude the existence of symmetrical reciprocal influence relationships in the real world. As Nagel (1975, p. 146) has observed, "it is important to distinguish the asymmetry of . . . social relations from the asymmetry of variables in a model. Only the latter is a defining characteristic of power."[10]

A second notion of inherent power asymmetry conceives of power as a situation in which A has more power over B than B has over A (Wrong, 1968, p. 673; Eckstein, 1973, p. 1146; Blau, 1964, pp. 117–18). Mutual influence of equal strength, according to Blau (1964, pp. 117–18), indicates lack of power. This notion of power necessitates comparison between A's power over B and B's power over A, thus implying that the scopes are comparable. Even seemingly comparable scopes, however, are likely to present problems for the power analyst. Soviet-American nuclear deterrence is often thought of as mutual influence. Even granting that these are roughly comparable scopes, there is something anomalous about describing this situation as characterized by an absence of power. Conventional social science usage would describe Soviet-American nuclear deterrence as two separate and relatively successful influence attempts. Two advantages of conventional usage are, first, that comparing scopes is optional rather than required and, second, that interdependence can be differentiated from independence. Many social scientists find it useful to distinguish between situations characterized by mutual influence among actors (interdependence) and situations characterized by a lack of influence among actors (independence).

Suppose A has the power to get B to play the piano (using positive and/or negative sanctions), while B has the power to get A to rake the yard (using positive and/or negative sanctions). Which one has more power? The notion of power as an imbalance of power between A and B requires an answer, but the conventional concept of power does not. If there were a generally agreed upon common denominator to which forms of power could be reduced, comparing different scopes would be easy; but no such standardized measuring rod for power exists (cf. Dahl and Lindblom, 1953, pp.

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[10] In my opinion, efforts to equate causal asymmetry with power asymmetry have been counterproductive to the extent that they have obscured one of Lasswell and Kaplan's most fundamental points—the need to treat power as a triadic relationship (Lasswell and Kaplan, 1950, p. 76). Simon (1957, p. 63) portrayed his work as "footnotes" to the work of Lasswell and Kaplan and attributed the notion of power asymmetry to them. This was unfortunate, since Lasswell and Kaplan were sensitive to reciprocity and symmetry in power relations and believed that power should not "be conceived as a unilateral relationship" (Lasswell and Kaplan, 1950, p. 201). Subsequent references to the "asymmetrical notion of power" in this article refer to the "imbalance-of-influence" sense of this term unless another meaning is specified. For a thorough discussion of causal asymmetry and power asymmetry, see Nagel (1975).

228–29; Dahl, 1976, pp. 32–36; Baldwin, 1971a). It is precisely because of the absence of close counterparts to money in noneconomic social interaction that scope must be specified in defining influence relationships.

The criticism of the asymmetrical notion of power presented here should be distinguished from that of Wrong (1968). Wrong objects to Blau's contention that power relations are always asymmetrical on the grounds that many power situations are characterized by reciprocal influence with regard to different scopes. Although Wrong (1968, pp. 673–74) admits that "asymmetry exists in each individual act-response sequence," he observes that "the actors continually alternate the roles of power holder and power subject in the total course of their interaction" so that a pattern may emerge in which one actor controls the other with respect to particular spheres of conduct, "while the other actor is regularly dominant in other areas of situated activity." Thus, Wrong posits a social situation in which each individual power relationship is asymmetrical (in that "the power holder exercises greater control over the behavior of the power subject than the reverse"), but in which A's ability to get B to play the piano is "balanced" by B's ability to get A to rake the yard.[11]

Wrong rightly criticizes Blau's failure to account for different scopes, but he stops short of objecting to the asymmetrical concept of power. In the absence of a common denominator of power values (i.e., a functional equivalent for money) for comparing different scopes, it is difficult to accept the notion of "individual act-response sequences" as asymmetrical. What standardized measuring rod are we to use in comparing A's control over B with B's control over A? When the United States tried to make North Vietnam give up its activities in South Vietnam, how successful was its influence attempt? Although the conventional concept of power permits a straightforward—but not necessarily easy—answer to this question, the "asymmetry imbalance" concept of power requires a comparison between the U.S. attempt to influence North Vietnam and the North Vietnamese attempt to get the Americans to go home. In other words, one is supposed to compare getting a country to give up claims to what it perceives as its own land and people (White, 1970) with getting a foreign power to stop intervening in one's country. Although others may describe differently the scopes of the American and North Vietnamese influence attempts, the point is that the two nations were engaged in fundamentally different kinds of influence attempts. Even those who deny the significance of the "territorial imperative" ought to admit the difference between aggression and resistance to aggression (cf. White, 1970; Ardrey, 1966; Lorenz, 1966), between making an influence attempt and resisting one.

In sum, there are two good reasons for rejecting the notion of power that implies a necessary imbalance of influence between A and B. First, it requires a comparison between influence attempts with different scopes in the absence of a generally agreed-upon criterion for making such comparisons. And second, the common-sense intuitive notion of power is captured at least as well by the concept of power developed by Dahl and others.

Homans' (1974, pp. 70–83) concept of asymmetrical power falls on the borderline between the "imbalance" notion and a third category based on unequal benefits; it thus deserves separate treatment. Homans begins by defining power as asymmetrical in the sense that the behavior of one of the parties to an exchange "changes in some sense more than the behavior of the other." Homans uses the example of an exchange of advice for approval,[12] thus raising the question of how to compare a change in approval with a change in advice in the absence of a common denominator of value. Since the difficulties of comparing different scopes have already been discussed, there is no need to belabor this point.

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[11] Wrong (1968, p. 674) uses the term "intercursive power" to refer to "relations between persons or groups in which the control of one person or group over the other with reference to a particular scope is balanced by the control of the other in a different scope." Nagel (1975, pp. 142–44) points out that such situations can be analyzed in terms of separate and distinct influence attempts by each party. In such situations A and B control *different* outcomes vis-à-vis each other. There are, of course, other situations in which A and B share power over a single outcome (Nagel, 1975, pp. 144–46). Thus, part of the difficulty in conceiving of routine commercial transactions as power relationships stems from the inclusion of both intercursive and shared power over a single outcome in such situations. The price (or exchange rate) is a single outcome over which the buyer and seller share power. Intercursive power, however, is also involved since the buyer is trying to obtain goods and/or services, while the seller is trying to obtain money. A full description of the power dimensions of a commercial transaction would have to include both kinds of power. "Buying a car" and "buying a car at a good price" are not necessarily (or even usually) the same thing—one has to work harder at the second than at the first.

[12] This example is used often by Homans. It refers to an experienced office worker's willingness to give helpful advice to less experienced fellow workers and their willingness to give gratitude and admiration in return.

Homans then shifts the focus of his discussion of power from the amount of behavior change to the "net reward" each party derives from the exchange. The "general condition that establishes interpersonal power," according to Homans, is that one party "gets less out of the exchange" than the other. Although one might think that the person "getting less" from the exchange is the weaker, this is not what Homans has in mind. Instead, he states–or restates–the "principle of least interest," in which "the person who is perceived by the other as the less interested, the more indifferent, to the exchange is apt to have the greater power." This principle, of course, has been discussed by Thibaut and Kelly (1959, p. 103) and Schelling (1960), and many would consider it a useful empirical observation about certain kinds of bargaining situations. For Homans, however, it is not merely a useful insight, it is "the one essential characteristic of power." He thus defines power as follows:

> When A's net reward–compared, that is, with his alternatives–in taking action that will reward B is less, at least as perceived by B, than B's net reward in taking action that will reward A, and B as a result changes his behavior in a way favorable to A, then A has exerted power over B.

In addition to the cumbersome wording, there are at least two difficulties with this definition. First, the role of B's perceptions is not clear. If B's perceptions of A's relative indifference are crucial, why even mention the real situation? If B's perceptions are what matter, why does Homans (1974, p. 85) insist that "it is necessary to know the payoffs for both parties, not just for one of them?" A second difficulty is the possibility that interpersonal comparisons of subjective value are implied by the need to compare A's reward with B's. Although Homans (1974, p. 74) explicitly denies that this problem exists, his explanation begs several questions. For example, Homans argues that objective indicators of net rewards can be found but then admits that such indicators can be manipulated by each party so as to mislead the other. Furthermore, Homans' argument that the actual participants in the power transaction can avoid direct interpersonal value comparisons is beside the point. The prohibition on such comparisons is a methodological constraint on social scientists, not on other people. In our everyday lives we can and do make methodologically unsound value comparisons all the time.

In addition to causal asymmetry, "imbalance" asymmetry, and "unequal benefit" asymmetry, some would say that power asymmetry resides in the uneven distribution of power resources. In every society some people have more power than others, and there is an implication of asymmetry in that fact. This concept is mentioned here only to show how appealing the idea of power as inherently asymmetrical can be. When someone observes that power is asymmetrical, everyone may nod; but they do not necessarily agree on the meaning of the message.

Thus far, objections to the concept of power as inherently asymmetrical have stemmed primarily from the relative absence of counterparts to money in social exchange and the consequent need to specify scope in defining and measuring power. Nagel (1968, pp. 135–37), however, has suggested still another objection, based on the *costs* of making influence attempts. Nagel points out that many influence situations require A to reward, punish, threaten, promise, furnish information, and/or monitor in order to get B to do X. Since "these behaviors of A are caused by B's reluctance to do X (or, more generally, by the improbability of B's doing X spontaneously)," Nagel suggests, "they indicate the scope of B's power over A."[13] Thus, when the opportunity costs of A's influence attempt are taken into account, the inherently asymmetrical concept of power is further called into question.

Consider two examples that Eckstein (1973, p. 1150) claims are "clearly asymmetric"–"the power of a criminal over his victim or the slow driver over the queues in his wake." If A must commit a criminal act in order to get B to do X, A has modified his behavior in what is likely to be a significant and costly way. Likewise, the slow driver can make those behind him late for work, but only by driving so slowly that he risks making himself late also. In such situations A influences B to do X, but B's reluctance to do X also affects A's behavior. If power costs are considered, such situations are not so "clearly asymmetric" as they appear to be.[14]

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[13] In a later work, Nagel (1975, p. 141 n.) modifies his position, arguing that "the effects a weaker actor may have upon a stronger cannot generally be construed as power because they are not necessarily the consequence of the weaker actor's preferences." For purposes of this discussion, however, the fact that such effects are sometimes a consequence of the weaker actor's preferences is sufficient to call into question the notion of power as inherently asymmetrical in the "imbalance" sense.

In sum, several notions of power as inherently asymmetrical have been examined. The causal asymmetry notion is valid but can be confusing when it is applied to power relations. The "imbalance" notion and the "unequal benefits" notion, however, have serious drawbacks. The concept of power developed by Dahl, Lasswell and Kaplan, and others involves causal asymmetry but allows for both symmetric and asymmetric power relationships. Since exchange relations connote reciprocity and the possibility—but not the necessity—of symmetry, the "imbalance" and "unequal benefits" notions of power as inherently asymmetrical are not well suited for inclusion in exchange models.[15]

Positive vs. Negative Sanctions

The distinction between positive and negative sanctions has been an important obstacle to incorporating a broad concept of power into social exchange models. Exchange is often depicted as mutually rewarding and beneficial, while power relations are often portrayed as based on negative sanctions and detrimental to the object of the influence attempt. A fundamental difference between Blau (1964) and Homans (1974) on this point should be noted. On the one hand, Blau (1964, pp. 91–92, 115–17) defines exchange in terms of positive sanctions and power in terms of negative sanctions. Homans (1974, p. 83), on the other hand, objects to such a view:

> Who shall say that a man who offers others good pay to do his bidding, and they jump at the chance, has not exerted power over them? The trouble is that in the everyday thinking of many of us we do not consider power to be really power unless it is accompanied by orders, threats, the imposition of wills, and resistance. We believe power to be inherently evil, though sometimes necessary. Yet none of these things is essential to power as we shall define it. Indeed, if we could count up all the examples of the exertion of power, we suspect that we should find the noncoercive form to be far more common than the coercive.

The initial requirements for integration of exchange analysis and power analysis are a concept of power broad enough to subsume positive sanctions and a concept of exchange broad enough to include negative sanctions. Homans (1974), Ilchman and Uphoff (1969), and Waldman (1972) have demonstrated that exchange models can be used to explain social interaction based on threats and/or punishments. Others (Dahl, 1968; Nagel, 1975; Oppenheim, forthcoming; Cartwright, 1965) have shown that power can be defined so as to allow for influence attempts based on positive sanctions. Such a broad concept of power has an important advantage over one based only on negative sanctions—it facilitates description of the full range of policy options available to A in making an influence attempt on B. As Harsanyi (1962, p. 69) has pointed out, this is "one of the main purposes for which social scientists use the concept of A's power over B." I have never come across a serious scholarly argument in favor of confining an examination of A's policy options to those based on negative sanctions. The distinction between positive and negative sanctions is useful (Baldwin, 1971b), but *it does not require us to differentiate* exchange from power.

Authority

It is sometimes suggested that authority—or legitimate power—presents especially difficult analytical problems, and much futile debate has focused on the similarities and differences between power and authority. At first glance, authority relations would seem quite different from exchange relations; but closer scrutiny discredits this first impression. Indeed, social exchange theory has made some of its most impressive contributions in discussions of authority and legitimacy (Blau, 1963; Waldman, 1972; Ilchman and Uphoff, 1969). This is not surprising when we consider the sensitivity of all social exchange theorists to the importance of positive sanctions in social life and the importance of such sanctions in determining legitimacy. The adage that "one can do anything with bayonets except sit on them" suggests that negative sanctions are less useful for acquiring legitimacy than are positive sanctions. One rarely builds a high degree of legitimacy by terror, coercion, punishment, threats, intimidation, and harassment.

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[14] On the importance of power costs, see Nagel (1968); Harsanyi (1962); Deutsch (1963, pp. 110–16); and Baldwin (1971c). Deutsch (1963, p. 115) quotes Booker T. Washington: "The only way the white man in the South can keep the Negro in the ditch is to stay in the ditch with him."

[15] For more comments on defining power in terms of asymmetry, see Cartwright (1959, pp. 197–98); Oppenheim, 1961, pp. 104–06); Harsanyi (1962); Dahlström (1966); Nagel (1968, pp. 135–37; 1975, pp. 141–53); and Dahl (1976, p. 50). It should be emphasized that no one advocates viewing power as inherently symmetrical. The fruitful alternative to the assumption of asymmetry is the assumption of nonsymmetry, which allows for both symmetrical and asymmetrical power relations.

Analysis of authority in terms of exchange concepts is hardly new. Social contract theorists, such as Thomas Hobbes and John Locke, placed social exchange at the heart of their explanations of political authority. They envisioned the public as giving political leaders obedience and legitimacy in return for effective performance of governmental duties. Moreover, should political leaders prove unable to perform such services, the public has the right to withdraw its allegiance and stop obeying.

Recent social exchange theorists' treatment of authority is remarkably consistent with the traditional social contract explanation. Blau (1963, p. 209), for example, describes authority relations from the perspective of the individual and of the collectivity. From the individual's standpoint, obedience to the law is exchanged for social approval from one's peers; thus, the exchange between the individual and the government is indirect. The collectivity of individuals, however, gives governmental leaders two things–prevailing compliance with lawful orders and a set of social norms that help enforce compliance–in direct exchange for the contribution to the common welfare furnished by political leaders. Thus, individual incentives to obey the law (authoritative commands) are seen as different from the incentives for the collectivity. Waldman (1972, pp. 89–117) places less emphasis on the difference between individual and group perspectives but still views legitimacy as something conferred on governments in exchange for satisfaction of basic desires for security, welfare, and dignity. Waldman (1972, p. 96) argues that his exchange analysis of legitimacy is "not inconsistent with other explanations of the phenomenon" and that it "refines and accounts for the validity of some of the classic explanations of authority."

Authority relations present no insurmountable analytical problems for exchange theorists. Explanation of authority in terms of exchange is an ancient, honorable, and still viable tradition.[16]

Conclusion

The purpose of this article is to evaluate the possibility and/or desirability of conceptual integration of two key social science terms–power and exchange. The discussion thus far has shown the possibility of using power terminology to describe exchange relations and the possibility of using exchange terminology to describe many kinds of power relations. In the remainder of the discussion I will consider Cartwright's (1965) criticisms of treating power as a kind of exchange and identify the potential *advantages* of such treatment. Proper evaluation, of course, would require identification of *disadvantages* and weighing them against the advantages. The disadvantages of using exchange models to study power, however, will not be considered here. This lacuna is not only due to space limitations but also to the principle of "comparative need," i.e., it is often implied that the advantages are nonexistent. I suspect that the final accounting will fail to yield a clear-cut answer regarding the wisdom of treating power in terms of exchange models. The concept of power may be more useful in looking at social relations from the standpoint of a single actor. Although this is a legitimate analytical focus for a social scientist, it is not the only legitimate one. The concept of exchange may be more useful when one wants to emphasize the interactive or reciprocal nature of the relationship. Thus, depending on one's analytical purpose, the relative desirability of emphasizing power or exchange may vary.

Cartwright (1965, pp. 16–18) observes that the "concept of exchange provides considerable insight into many aspects of the influence process," but he sees severe difficulties in attempting to conceive of all influence processes in this way. These difficulties are as follows:

1. **Noncontingent Means.** When A's sanctions are contingent on B's compliance, it is relatively easy to think of the influence process in terms of exchange. However, when A rewards or punishes B in a noncontingent way–by turning up the heat so as to increase the probability that B will remove a sweater or by giving B money so as to increase the probability that B will go to a movie–the exchange analogy is less obvious.

Comment: This is a valid criticism, which suggests that some kinds of power relations cannot be described in terms of exchange. It should be noted, however, that failure to account for influence attempts based on noncontingent means is a weakness found in many discussions of power regardless of whether or not they are based on exchange.[17]

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[16] For a discussion of the analogy between the economist's concept of "liquidity" and the political scientist's concept of "legitimacy" see Baldwin (1971a).

2. No Resource Transfer. In A's attempt to influence B, "resources" may be consumed, transferred; or no change in ownership may occur.

Comment: The validity of this criticism is questionable, since resources may be exchanged in subtle ways. Two examples used by Cartwright to illustrate influence without resource transfer are as follows: First, distinguished citizens who "lend their names" to fund-raising organizations are depicted as giving their names but also keeping them. Social exchange theory, however, can describe this situation quite well as a situation in which the distinguished citizens receive enhanced status in return for allowing their names to be used by fund-raising organizations. Second, the supervisor who promises an employee a favor but fails to fulfill the promise is described as keeping possession of the resource. Once again, exchange theory would explain this as an employee having exchanged compliance in return for a promised favor. The subsequent breaking of the promise is beside the point. It is the value of the promise at the moment of exchange that matters, not its later devaluation. Exchange is not necessarily equitable; even those who get gypped are participating in an exchange.[18]

3. No Commensurate Units of Value. Even when exchange may plausibly be said to occur, it is often most difficult to describe the exchange in commensurate units of value.

Comment: This is true, but it does not constitute a serious weakness in exchange models. The exchange of approval for advice, of compliance for money, or of one favor for another does not require measurement in terms of commensurate units of value—at least not so long as we speak of it as direct exchange (barter). It is only when indirect exchange and the fairness of exchange are discussed that standardized measures of value begin to matter. One of the exchange theorists' most important insights concerns the way societal norms function as *primitive* measuring rods that make indirect social exchange possible.[19] How do we know how much compliance it is appropriate to give police officers? Social norms embodied in laws tell us. How do you know whether it is "fair" for your neighbor to ask to sleep with your spouse in return for lending you a lawnmower? Social norms tell you. And so on. The lack of commensurate units of value for measuring exchange is not a serious problem[20] for exchange models of power unless they employ the notion of power as inherently asymmetrical in the "power imbalance" sense discussed previously.

Despite his criticism of the attempts to marry exchange and power, Cartwright contends that "it would be premature to conclude that the effort is necessarily doomed to failure." The fact that the effort is not doomed to fail, of course, does not justify the undertaking. Let us look at some of the potential advantages of treating power—at least some kinds of power—as exchange relationships.

Several advantages of treating power as exchange can be identified. Since most exchange theorists have thus far employed an asymmetric concept of power, however, some of these advantages are potential rather than actual. These potential advantages, then, pertain to treating the conventional social science concept of power in an exchange perspective.

First, treating power as exchange would emphasize the relational nature of power, since almost everyone thinks of exchange as relational. Second, emphasis on exchange and the possible reciprocity thereby implied should help students of organization theory in overcoming the often-lamented tendency to view power as vertical, hierarchical, asymmetrical, and unilateral (Cartwright, 1965, p. 2; Palumbo, 1975, p. 353). Third, an exchange perspective on power should inhibit the tendency to view all power relations as exploitative. A zero-sum concept of power, in which A is the "victor" and B is the "victim," is not adequate for describing many kinds of power relationships. Fourth, an exchange orientation should help sensitize power theorists to the important role of costs. This has been a traditional blind spot for students of power (Baldwin, 1971c), but it has been one of the social exchange theorists' strengths that they have made us aware of many of the less obvious costs associated with various human choices (cf. Chadwick-Jones, 1976, pp. 178–80). Fifth, the propensity of power theorists to ignore positive sanctions might be offset by the exchange theorists' emphasis on rewards (Baldwin, 1971b). And sixth, integration of such basic concepts as power and exchange could be a step toward conceptual unification of the social sciences, thus facilitating communication among economists, political scientists, sociologists, and social psychologists.

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[17] Harsanyi's (1962, p. 71) treatment of influence attempts based on noncontingent means is especially useful. He notes, moreover, that he has given this case extra attention because it "is often overlooked in the literature." See also Baldwin (1971d, pp. 476–77).

[18] Cartwright's implication that a broken promise has no value is also unacceptable. Anyone with children knows that the breaking of last week's promise to take them to the movies makes it much harder to break this week's promise to take them. Broken promises can be valuable power resources, and not only for children.

[19] There is no contradiction here with the previous emphasis on the differences between the media of social exchange and money. Both social norms and money serve as media of exchange, but social norms are very primitive while money is very sophisticated. Social norms are like money in the same way that a horse is like a new Cadillac. Both are means of transportation, but what a difference!

[20] In a sense the lack of a standardized measure of value is a "serious problem" for all power analysis. It is not, however, a problem peculiar to exchange analysis.

A final and fundamental point should be of special interest to political scientists. Much contemporary thinking about politics tends to view power relations as conflictual, negative, exploitative, coercive, and unpleasant from the standpoint of the one who is influenced. Exchange relations, however, tend to be depicted as cooperative, positive, beneficial, voluntary, and pleasant. The basic conception of the relation between the people and the state that emerges from such modes of thinking is likely to emphasize autonomous individuals tolerating government as a necessary evil. By contrast, a power-exchange theory of politics, such as that proposed by G. E. G. Catlin (1927, 1930), suggests that much of the resentment, suspicion, and vilification of politics and politicians is unjustified. The political process, Catlin implies, is not nearly so asymmetric as it appears to the ordinary person. Politicians provide valuable services and should not be viewed as parasites who exploit society.

> The politician, as political capitalist, assumes responsibility and expends his energies in the labour of government in return for the interest of power. And he does this entirely legitimately insofar as the power he enjoys is the power of moulding policy which accrues to a man who has the intelligence to provide men politically with the social adjustments, securities, and facilities which they want but are unable themselves to procure. The social service of the statesman is at least as high and as much deserving of recompense as that of the financier (Catlin, 1930, pp. 184–85).

The question of what constitutes an "accurate" or "desirable" basic concept of the relation between the government and the governed is beyond the scope of this article; but if the way we think about power affects the way we conceive of this basic social relation, then the stakes involved in deciding how to treat power and exchange are far greater than first appears. The question of whether one agrees with Eckstein (1973, p. 1159) that exchanges are elements of "dissonance" or "impurity" in the political process or whether one agrees with Dahl (1976, p. 50) that exchanges "are as ubiquitous in political as in economic life" deserves serious consideration by every political scientist.

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The Expansion of the Public Economy: A Comparative Analysis*

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In spite of the traditional legitimacy accorded the market mechanism of the private sector in advanced capitalist nations, governments in those nations have become more influential as providers of social services and income supplements, producers of goods, managers of the economy, and investors of capital. And in order to finance these various activities the revenues of public authorities have increased dramatically—to a point where they are now equivalent to one-third to one-half of a nation's economic product.

This growth in governmental activity in advanced capitalist society is examined by considering the causes, and some of the consequences, of the expansion of the public economy—defined, following Schumpeter's discussion of the "tax state," in terms of the extractive role of government. The primary concern of this article is to discover why some nations have experienced a far greater rate of increase in recent years and, as a result, have a much larger public economy than other nations. Five types of explanation are elaborated to account for the growth of the scope of governmental activity: (1) the level and rate of growth in the economic product; (2) the degree to which the fiscal structure of a nation relies on indirect, or "invisible," taxes; (3) politics—in particular the partisan composition of government and the frequency of electoral competition; (4) the institutional structure of government; and (5) the degree of exposure of the economy to the international marketplace. The article evaluates the five explanations with data for 18 nations, and concludes by discussing some implications of the analysis.

During the three decades following World War II, the role of government in most advanced capitalist economies increased dramatically. With the maturation of the "welfare state," governments increased their provision of social services and income transfers for the unemployed, the sick, the elderly, and the poor. Furthermore, governments have become important producers of goods, and in several European nations publicly owned corporations dominate the petroleum, automotive, and transportation industries. In addition, by using a variety of fiscal and monetary instruments such as public spending programs, taxes, and discount rates, governments have attempted to manipulate the levels of unemployment and inflation and dampen the effects of business cycles. They have also sought to guide the long-term development of the economy through the creation of planning institutions and, occasionally, through their control of the assets of financial institutions. And in order to finance their activities in each of these domains, governments have instituted new taxes and raised the levels of old taxes. Indeed, the growth of the extractive role of public authorities has been so great that Schumpeter's words, written half a century ago and pertaining to the historical development of Europe, seem even more appropriate today: "Tax bill in hand, the state penetrated the private economies and won increasing dominion over them" (1918, p. 19).

This article explores the causes of the expansion of the extractive role of government during the recent past in 18 relatively developed capitalist nations. The first section presents several alternative explanations of that expansion. Following the argument developed, rather imprecisely, by Wagner (1883) in the late nineteenth century, most economists have concentrated on the impact of economic growth in accounting for the inexorable increase in government activity. In addition to outlining the Wagnerian argument, I will discuss several alternative causes of the expansion of public revenues. In particular, the analysis examines the impact on taxation of two aspects of the policy-making environment whose effects have been frequently disputed or neglected in many comparative studies of public policy: (1) the electoral politics of a nation—for example, the existence of electoral competition and the partisan composition of government; and (2) the institutional structure of government—for example, the existence of unitary or federal

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government, and the extent of fiscal centralization. The analysis also considers the extent to which the scope of government activity within a nation is influenced by the position of the domestic economy vis-à-vis the world economy. In this regard, the analysis investigates whether the openness, or exposure, of a nation's economy to the international marketplace stimulates an expansion in the role of government.

The several explanations of the expansion of the public economy[1] will be evaluated by analyzing the experience of 18 nations since 1960. All 18 have capitalist economies; this criterion allows the inclusion of nations for which the expansion of the public economy might represent a source of tension and conflict with the traditional reliance on, and legitimation of, the market economy for the allocation of goods. The nations are: the United States, Canada, Britain, Ireland, Australia, Japan, the Federal Republic of Germany, France, Italy, Spain, the Netherlands, Austria, Belgium, Switzerland, Sweden, Norway, Denmark, and Finland.

Measuring the Scope of the Public Economy

The scope of activity of public authorities can be described by enumerating the programs and types of expenditures carried out by government (King, 1973). However, in the contemporary era in the nations considered here, the increase in the level of expenditure and the multiple uses of funds has been accompanied by an expansion of the revenue-generating capacity of government. Although imbalances occasionally occur between the aggregate totals of all government revenues and expenditures, the two have usually moved in tandem. Thus, the scope of the public economy can be compared as well by considering the revenues of governments rather than their expenditures—that is, by considering the extractive aspect of government.[2] The public economy is defined in terms of the total of all revenues obtained by all levels of government in a nation. Included are all direct taxes, e.g., those on personal and corporate incomes and property; all indirect taxes, e.g., those on sales and value added; all social insurance contributions by employers and employees; and all other fees, taxes, rents, and withdrawals from enterprises which flow into governmental treasuries. To convey the relative importance of the funds which are appropriated (and subsequently distributed) by public authorities, and to control for the obvious differences among nations in the size of the economy, I have calculated the ratio of all governmental revenues to Gross Domestic Product (GDP). This ratio, calculated for all years between 1960 and 1975 (the latest for which data are available), is treated as a measure of the scope of the public economy.[3] In order to measure and compare across nations the degree of expansion in the scope of the public economy, I have calculated the first-order difference in this ratio for the earliest and latest years.[4]

There has been significant variation in both the level and rate of change in the scope of the public economy among these 18 nations. In 1960, for example, the scope of the public economy, which averaged 28.5 percent of GDP, varied from 18 percent in Spain to 35.4 in Germany. By 1975, the average scope of the public economy increased to 38.5 percent of GDP and the variation among the nations also increased, ranging from 23.5 percent in Japan to 53.5 percent in the Netherlands. Thus, while there was a general trend of expansion of the public economy at work in all the nations, there were also great differences among them in the rate of expansion. Why that rate of expansion varied as much as it did is the question to which we now turn.

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[1] The term "public economy" refers here to that portion of a nation's economic product which is consumed or distributed by all public authorities.

[2] In most nations, during most years, the total of all governments' revenues exceeds the total of all governments' expenditures. During the world recession of 1975, the aggregate of all governments' expenditures exceeded all revenues in six nations—the United States, Belgium, Germany, Ireland, Italy, and Britain. In the prior half-decade (1970–74), however, only two nations (the United States in 1971 and Italy in 1971–74) experienced an aggregate deficit. Thus, a measure of the public economy based on revenues is less likely to understate the fiscal scope of governmental activity than one based on expenditures. See Organisation for Economic Co-operation and Development (O.E.C.D.) (1977a).

[3] The data were obtained from O.E.C.D. (1973, 1977a). The measure includes withdrawals from, but not operating revenues of, public enterprises, which in some nations (for example, Austria, Finland, Britain, and Italy) are among the largest industrial firms.

[4] First-order differences, rather than percentage changes, are used in order to avoid artificially deflating the magnitude of change in nations with a relatively *high* value in the initial year. To control for the tendency of first-order differences to deflate the magnitude of change in nations with relatively *low* initial values, regression analyses will include both the value in the initial year and the measure of change.

Five Explanations of the Expansion of the Public Economy

Several distinct explanations of why the scope of the public economy changes over time can be identified. This section elaborates five types of explanation–one economic, the second fiscal, the third political, the fourth institutional, and the fifth international in character–and derives predictions to account for the considerable differences in the rate of expansion of the public economy in the 18 nations.

The Economic Explanation. The most frequently mentioned explanation of the increase in the scope of the public economy is that derived from Wagner's "law of expanding state activity" (1883, pp. 1–8). The "law" holds that, among European nations, the "pressure for social progress" leads inevitably to the growth of the public sector. Writing in the midst of a period of rapid urbanization and industrialization, and just as Bismarck was developing the first programs of the welfare state, Wagner recognized the growing role of the state as a provider of social overhead investments in such areas as transportation and education and the need, even in an authoritarian state, for the state to retain legitimacy by providing public funds to compensate for the human costs of economic development.

As elaborated by numerous scholars of public finance,[5] Wagner's "law" suggests that citizen's demands for services and willingness to pay taxes are income-elastic, and therefore bound to increase with the increase in economic affluence. If this "law" is correct, one would expect that, in comparing the experiences of a number of nations, the greater the increment in economic affluence of a nation during a given period, the greater the expansion of the public economy.

Several scholars have rejected the logic and evidence in support of Wagner's "law." Bird (1971, p. 19), Musgrave (1969, pp. 112–13), and Gupta (1968) find that any positive cross-national relationship between economic growth and government share in the economic product disappears when analysis is confined to the wealthier nations of the world. Apparently there exists an upper threshold to the scope of the public economy; beyond certain levels of income, international demonstration effects and internally derived perceptions of marginal benefit are less likely to generate increases in spending. Peacock and Wiseman (1967) also reject the "historical determinism" implicit in Wagner's "law" and in their discussion of the "displacement effect" emphasize the importance of crises such as war and depression in inducing infrequent but large changes in the tolerable burden of taxation. This provides little assistance in explaining a secular upward trend during periods of prosperity and peace (both of which existed in most of the 18 nations during most of the period between 1960 and 1975). However, the Peacock and Wiseman argument, taken with the findings of Bird, Musgrave, and Gupta, might lead one to expect a negligible relationship between economic growth and state expansion.

Wildavsky (1975, pp. 232–35) provides a third perspective on the economic sources of public sector expansion. In what might be termed a "counter-Wagner" law, Wildavsky suggests that the degree of expansion in the scope of the public economy varies inversely, rather than directly, with economic growth. Where national affluence increases very rapidly, as in Japan, any increased demand for public funds can be met by the added revenues obtained by applying a constant public share to a larger economic product. But where economic growth is so modest that it generates insufficient revenues to meet demands for additional public goods, as in Britain, those demands must be met through an expansion of the public share of the economic product. In short, Wildavsky's argument would predict that the relationship between growth and public sector expansion would be negative, indicating the greatest expansion of the public economy in low-growth nations.[6]

The Fiscal Explanation. The second type of explanation of public sector growth is fiscal in nature. As elaborated by Downs (1960) and Buchanan and Wagner (1977), this perspective emphasizes the structure of the system of revenue generation as a determinant of how much revenue can be raised. Downs argues that because public goods are inherently nondivisible, costs and benefits are not directly linked. Benefits frequently are uncertain (as in preventive or long-term policies). In addition, public goods are, when taken as a whole, inevitably suboptimal, since each citizen will pay for some programs that provide no individual benefit. As a result, the costs, i.e., taxes, are perceived to exceed the benefits of public goods. Therefore, it is only when public officials can conceal the costs of policies in a "fiscal illusion" that they can spend large amounts without incurring the wrath of the electorate. As Buchanan and Wagner (1977) argue, "complex and indirect payment structures create a fiscal illusion that will systematically produce higher levels of public outlay than those that would be observed under simple payments structures" (p. 129).

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[5] See Bird (1970, p. 70; 1971), Musgrave (1969, p. 74), and Gupta (1968).

[6] The data for the level of per capita Gross Domestic Product are reported in United Nations (1976, pp. 701–02). The data for the average annual increase in real GDP were obtained from O.E.C.D. (1977b, Technical Annex).

The major form of tax concealment, according to Downs (1960, p. 558), Wildavsky (1975, pp. 235–39), and Wilensky (1975, p. 52), is indirect taxation. In addition to taxes on value added or sales, relatively invisible forms of revenue generation might include taxes which are paid before individuals receive income, e.g., social insurance contributions by employees, and taxes which can be passed on to third parties, e.g., the social insurance contributions of employers. Applying this "fiscal illusion" argument to the present analysis, one might expect that the nations with the largest increase in the scope of the public economy would be ones in which there is a large and increasing reliance on indirect taxes and social insurance contributions.[7]

The Political Explanation. A third type of explanation of the expansion of the public economy involves politics. The impact of politics on the scope of the public sector has seldom been recognized, in spite of Downs' assertion (1960, p. 541) that "in a democratic society, the division of resources between the public and private sector is roughly determined by the desires of the electorate." Recently, however, studies by Kramer (1971), Nordhaus (1975), Tufte (1975, 1978), and Hibbs (1978) have suggested that politics–especially electoral politics–exerts a significant influence on the public economy. Two aspects of politics may influence the magnitude of expansion of the public economy: (1) the effect of electoral competition in "bidding up" the scope of expenditure programs; and (2) the effect of variations in the partisan composition, and presumably the ideological preferences, of government.

In his revision of the classical doctrine of democracy, Schumpeter (1950, p. 269) defined democracy in terms of electoral competition: "the democratic method is that institutional arrangement for arriving at political decisions in which individuals acquire the power to decide by means of a competitive struggle for the people's vote." As elaborated by Downs (1957), this theory of democracy implies that the contenders for political office alter their programs in order to enhance their electoral appeal. An important weapon in this competitive struggle is the public economy: some political contenders will attempt to garner votes by promising cuts in taxes; others will promise increases in spending; and others will promise both (see Downs, 1960; Buchanan and Wagner, 1977).

Ever since Key (1949) noted the propensity of political opponents to appeal to the "have-not" voters by promising more spending, scholars have been virtually unanimous in attributing to electoral competition an expansionist impulse (see Brittan, 1975, pp. 139–40). Empirical studies of the American electorate by Kramer (1971) and Tufte (1975) have demonstrated that voters have tended to provide short-term electoral rewards to incumbents who can effect, through their tax, fiscal, and monetary policies, increases in real personal income. And as Tufte (1978, Ch. 2) demonstrates, because incumbents are aware of this relationship, most adopt policies in anticipation of elections which stimulate the economy and increase personal income by pumping funds into the economy. As a result, periodic electoral competition frequently produces a long-term cyclical effect on the economy. The most important consequence, for our concerns here, is that this "political business cycle" is marked by increased spending and other reflationary policies in the period immediately before and after an election (Nordhaus, 1975; Lindbeck, 1976).

The existence of a "political business cycle," the likelihood that incumbents will attempt to use spending policies to enhance their support in anticipation of elections, as well as the tendency of opposition parties to build electoral support by promising more spending on particular government policies, all tend to suggest that public spending increases at an unusually rapid rate immediately before elections (and immediately afterward, if the opposition takes control of government). One application of this argument to the cross-national perspective is to consider the impact of variations among nations in the frequency of national elections. Since the measure of the scope of the public economy relies on revenues (i.e., taxes) rather than expenditures, frequent electoral competition might be expected to dampen the growth of the public economy. However, because revenues and expenditures moved in tandem in most, if not all, of the nations during the period under consideration, the effects of electoral competition on public spending tended to be reflected in tax increases (often in the post-election period). Therefore, one might expect that nations with frequent elections during 1960–75 experienced larger increases in the public economy than nations with infrequent elections, since there were more opportunities for government and opposition elites to indulge in "competitive bidding."[8]

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[7] Data pertaining to the structure of governments' revenues, and the portion that was generated through indirect taxes and social security contributions, are reported in O.E.C.D. (1973, 1977a).

One of the most contested issues in political science is whether or not partisanship influences public policy, and if so, how. Many scholars conclude that parties do not differ significantly in their positions on major issues. Proponents of the "decline of ideology" thesis argue, for example, that parties–particularly those on the left–have forsaken traditional ideologies in order to assemble larger electorates.[9] Even if parties resist the temptation of assembling socially and ideologically heterogeneous electorates and retain some measure of ideological distinctiveness, they may be unable to implement their preferences when in office. The complexities of the internal processes of revenue and spending decisions (Klein, 1976), the vagueness of campaign proposals and the frequent lack of experience, and turnover, of ministers (Gordon, 1971), the importance of policy professionals in the civil service (Heclo, 1974, p. 301), the impulse of incrementalism and the tendency to follow established routines (Wildavsky, 1974), as well as the occasional movement by governments in directions not consistent with their traditional ideologies (King, 1969, p. 136) all do much to circumscribe the impact of party. Largely for these reasons, King concludes that the policy role of parties is "sharply restricted.... While organized party generally remains one of the forces ... in the formation of public policy ... it has never been the only one, and there is reason to suppose that in many countries in the late 1960's it is not even a major one" (pp. 136–37).

In spite of the support which can be mustered for the skeptical view of the importance of parties in the policy process, some evidence suggests that parties may be relevant in defining the scope of the public economy. For example, Hibbs (1978) contends that strike activity decreased in European nations where Social Democratic and Labor parties increased their representation in cabinets in the 1930s and 1940s. This decrease occurred, says Hibbs (p. 165), because of the propensity of leftist parties to provide funding for new and expanded welfare programs. This caused a much larger portion of the national income to flow through the public sector, thereby shifting the locus of the distributional struggle from the private sector, where labor and capital compete through industrial conflict, to the public sector, where these economic actors compete through electoral mobilization and political bargaining with each other and with government.[10]

The budgetary studies of Davis, Dempster, and Wildavsky (1966, 1974) provide additional evidence that the partisanship of government may influence public policy. In their first study, the authors found "shift points" in the incremental drift of spending which coincided with changes in the control of the White House. Wildavsky's analysis (1975, p. 371) of British budgets during 1964–74 found that the direction of the trend line of public spending shifted when the Conservatives replaced the Labour party in government in 1970.[11] Also, Davis,

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[8] During 1960–75, the number of national legislative elections ranged from zero in Spain to seven in Australia and Denmark. During this period, of course, Spain was nondemocratic; thus our arguments concerning the impact of electoral competition on fiscal policy are irrelevant for that case. For purposes of comparability with the other nations, we treat only the four American presidential elections rather than all eight congressional elections. It is, of course, true that the argument regarding the effect of electoral competition on fiscal policy is relevant to the latter elections; see Kramer (1971) and Tufte (1975).

[9] See Lipset (1960, pp. 439–56) and Bell (1960). For rejoinders, see LaPalombara (1966) and Putnam (1973).

[10] The most dramatic instances of reduced industrial conflict after World War II are found in Scandinavia. For discussions of the institutionalization of industrial conflict in those nations, see Elvander (1974) and Ingham (1974).

[11] The effect is less pronounced when one considers the trend in British expenditures, in part because the government of Edward Heath ran down the surplus of

Dempster, and Wildavsky (1974) found that the magnitude of the increments granted to American federal agencies was influenced by such partisan variables as the strength of the liberal wing of the Democrats in Congress and Democratic control of the presidency.

The Hibbs and Wildavsky studies suggest that as the partisan composition of government varies over time, within a nation, and among nations, so too the priorities and substance of policy vary, including the definition by governments of the "proper" scope of the public economy. One might expect, therefore, that the considerable variation among nations in the rate of expansion of the public economy reflects differences in the frequency of control of national governments over a period of years by parties which, in general, favor that expansion. Following Downs (1957, p. 116), who viewed leftist parties as more favorable than others to the extension of governmental intervention in the economy, one might expect that the rate of expansion of the public economy was positively associated with the extent to which governments relied for their support on leftist parties during 1960–75.[12]

The Institutional Explanation. A fourth type of explanation of the expansion of the public economy involves the institutional structure of government. Two aspects of that structure are considered here: (1) the formal relationship among levels of government within a nation—in particular, the existence of multiple, independent centers of public authority; and (2) the degree of fiscal centralization.

Downs (1964), Niskanen (1971), Wildavsky (1974), and Tarschys (1975) argue that government bureaucracies develop internal pressures for self-aggrandizement and expansion. If that is true, then a multiplicity of autonomous governmental bureaucracies would enhance this tendency. Thus, in nations where no single authority controls the bulk of public spending—where, in other words, spending authority is fragmented—and where the institutional structure guarantees that some units and levels spend funds that were raised by other units or levels (Tarschys, 1975, p. 25), the rate of increase in spending should be unusually high. The institutional arrangement of government which most closely approximates this situation is federalism: that arrangement provides considerable autonomy for subnational and local governments, fragments the control of public spending, allows some levels or units the luxury of spending funds which have been raised by other levels or units, and multiplies the number of self-aggrandizing bureaucracies. Therefore, one might expect that nations with a federal structure of government experienced larger increases in the scope of the public economy than those with a unitary structure.

A second aspect of the institutional structure of government that may influence the expansion of the public economy is the degree of fiscal centralization. Centralization, defined here as the proportion of all governments' revenues generated by the central government, reflects, to some extent, the formal institutional structure of government; nations with *unitary government tend to be more centralized* than federal nations. Nevertheless, the degree of centralization varies widely among both the

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funds allocated to capital formation. The result was a marked increase in borrowings from abroad, beginning in 1972, and actual deficits beginning in 1975. See Klein (1976, p. 418) and Brittan (1975).

[12] Leftist parties are defined as those which are Social Democratic, Labor, or Communist. Thus, the American Democratic party is not considered to be leftist; it is, of course, true that the party is to the left of the Republican party and that it does matter whether Republicans or Democrats control the executive and legislative branches. (In this regard, see Tufte [1978, pp. 71–83].) Nevertheless, it is also true that it differs considerably from most European Socialist and Social Democratic-Labor parties—for example, in its electoral support among middle-class voters and in its unusually large body of self-declared "middle-of-the-roaders" and "conservatives." See, on the former point, Hamilton (1972, pp. 190–93). In regard to the latter point, Flanigan and Zingale (1975, p. 114) find that 66 percent of 1972 Democratic identifiers called themselves "conservative" or "middle-of-the-road" rather than "liberal." The measure of the partisan composition of government was calculated as follows: in each year, I summed the votes received in the previous national legislative election by all parties participating in government (i.e., holding cabinet positions) and divided the total into the number of votes received by all Social Democratic, Socialist, Labor, Communist, and smaller leftist parties (such as the Dutch Radicals and the Danish Socialist Peoples' parties) that were participants in government. This proportion ranges from 0 (when no leftist parties were in government) to 100 (when the government was composed entirely of leftist parties). I then summed the values for the 16 years between 1960–75 and calculated the average. That average represents the average proportion of the government's electoral base that was accounted for by leftist parties. It is highly correlated (on the order of $r = .99$) with a measure of the proportion of Cabinet portfolios held by leftist parties. The electoral data were obtained from Mackie and Rose (1974, 1975, 1976).

federal and the nonfederal nations considered here, and it is plausible to think that it exerts an impact on the scope of the public economy that is independent of the formal structure of government.[13]

Several studies conclude that government spending tends to increase most rapidly at subnational and local levels of government. Wagner (1883, p. 8) was perhaps the first to note that local authorities' requirements for funds were most likely to expand when administration was decentralized. Recent studies of the United States (Freeman, 1975, p. 208), Britain (Bacon and Eltis, 1976), and Sweden (Tarschys, 1975, p. 25) have found that increases in public spending were greater at the subnational and local, rather than at the national, levels of government. Heidenheimer (1975, p. 28) suggests a reason for these findings in his comparison of the difference in spending for health care in Britain and Sweden: "The monopolistic control of the national Treasury and the Health Ministry over financing sources enabled Britain, much better than Sweden [where health financing is decentralized and the responsibility of the counties], to hold down the proportion of national income allocated to the health sector." In other words, the ability of central government decision makers to oversee spending and, presumably, their awareness of the cost-benefit tradeoffs among policy sectors serve to limit aggregate spending. These studies support a contention that, for the same reasons mentioned in regard to the impact of federalism, relatively decentralized nations experienced larger increases in the scope of the public economy than did highly centralized nations.

The International Explanation. The four alternative explanations of the expansion of the public economy presented thus far share a common element. The rate of economic growth, the degree of "invisibility" in the revenue-generating mechanisms of the state, electoral politics, and the institutional structure of government all involve internal aspects of nations. However, nations are not wholly autonomous and entirely independent of the external world. In fact, certain nations are highly dependent on their external environments as markets for export goods or sources of capital.[14] To the extent that there is a high degree of substitution of foreign and domestic goods, with domestic prices of commodities, labor, and capital established by supply and demand in the international rather than the domestic market, these economies are "open" (see Lindbeck, 1976, p. 2). They are, in other words, exposed to pressures on markets and prices which are transmitted from other nations via international exchange.

The concept of the open economy is applicable, in varying degrees, to almost all of the advanced capitalist societies considered here, but it is especially relevant for the smaller nations. As Dahl and Tufte (1973) note, trade dependence—one aspect of openness—is strongly, and inversely, related to the size of a nation:

> By free trade, small political systems can achieve the same economies of scale as large systems.... [Thus] a partial solution to the problem for small systems [is] to engage in foreign trade.... In general, the smaller a political system the higher the proportion of foreign trade to total trade.... Few relationships with size hold up more uniformly than this one (p. 115).

In several of the smaller nations considered here, such as Belgium and the Netherlands, the value of imports and exports is almost as large as the Gross Domestic Product. In fact, the value of trade exceeded 50 percent of GDP in 12 of the 18 nations in 1975.[15] Even in the larger nations, such as the United States and Japan, however, the economies are not impervious to the international economy. Like the smaller nations, they depend on external producers for important commodities such as oil and other raw materials, and they depend on external consumers to provide the markets for export goods.

The most important political consequence of an "open" economy is the constraint it imposes on the effectiveness of a variety of macroeconomic policies (see Krasner, 1976, p. 319). To quote Dahl and Tufte again (1973, pp. 116, 130):

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[13] The measure of the formal structure of government is binary: 0 for unitary nations; 1 for federal nations. The measure of centralization was obtained from O.E.C.D. (1973). The degree of centralization in 1960 for the federal nations varied between 38.2 percent (Germany) and 79.7 percent (Australia). Among the unitary nations, the measure of centralization varied from 59 percent (Italy) to 85.8 percent (Ireland). The correlation between the measure of federalism and that of centralization is r = –.53.

[14] See Lindbeck (1975, 1976, 1977); Aukrust (1977); Cooper (1972); and Keohane and Nye (1977).

[15] The figures refer to exports and imports of all goods and services, and are derived from data reported in O.E.C.D. (1973, 1977a).

> economies of scale tend to erode the independence and autonomy of the smaller democracy, making it dependent—officially or not—on the actions of people outside the country.... In order to develop and maintain a relatively high standard of living, the small country must go beyond its own boundaries in search of markets, and often in search of raw materials, labor, and capital investment as well. As a consequence, the small country is highly dependent on the behavior of foreign actors not subject to its authority.

Lindbeck (1975, 1977) and Aukrust (1977) have noted the several ways in which the trade dependence of the Scandinavian economies has limited the ability of national officials to manage aggregate demand and control inflation. Aggregate demand in small, open economies is, in part, a function of the demand in the world market for domestically produced export goods; thus, unemployment, which is a function of demand relative to operating capacity in the economy, is somewhat uncontrollable. And since price levels in export industries are set in the world market, they may not move in accordance with changes in domestic costs, causing either low profitability (if external demand, and prices, do not increase with increases in domestic costs) or high profitability (if external demand, and prices, increase more than domestic costs). Whether export profits are small or large, they may destabilize the economy: a lack of profits in the export sector may cause a reduction in the funds available for capital investment and, ultimately, a reduction in the rate of growth; a high level of export profits may, on the other hand, contribute to inflation, since they may be used to justify higher wages in the export industries and these wage increases may, in turn, be generalized to the nonexport sector through centralized collective bargaining.[16]

Just as a high degree of dependence on the international economy for markets for export industries may limit a government's ability to manage aggregate demand and control levels of unemployment and capital formation, a high degree of penetration of the domestic market by external producers also limits the control by the national government over the economy. High levels of imports tend to remove decisions regarding the production, and pricing, of goods for domestic consumption to external actors. In addition, import penetration may transmit inflationary pressures from the rest of the world—particularly if the domestic demand for particular commodities is relatively inelastic with regard to price, as, for example, oil (see Aukrust, 1977; Lindbeck, 1975). Furthermore, high levels of imports may contribute to balance of payments deficits, the solution of which usually requires the funding of deficits with foreign borrowings, devaluation, and the institution of deflationary "austerity" programs—all of which may be unattractive to national policy makers.

Several scholars have suggested that the vulnerability of the open economy can be lessened by an assertion of the role of the state. Gilpin (1975, p. 45) concludes, for example, that a likely result of the national anxieties and insecurities produced by economic interdependence will be a resurgence of mercantilism—that is, of states intervening between the domestic and international economy on behalf of national economic objectives. Myrdal (1960) suggested a similar development nearly two decades ago when noting that in order to protect national objectives of internal stability, employment of workers, and undisturbed domestic production and consumption in a world of more chaotic economic relations, "all states have felt themselves compelled to undertake new, radical intervention, not only in the sphere of their foreign trade and exchange relations, but also in other sectors of the national economy" (pp. 70–72). More recently, Lehmbruch (1977) reached a similar conclusion, arguing that

> in a national economy subject to strong interpenetration with international markets, monetary policies often are of rather limited effectiveness. The same is more and more true of "Keynesian" techniques of macroeconomic budgetary and fiscal demand management.... Faced with "control deficits" of this sort, governments increasingly turn to more direct attempts at influencing the economic behavior of business and/or labor (p. 98).

Lindbeck (1975, p. 56) argues that governments can dampen the effects of the open economy on production, employment, and consumption by increasing the scope of the public economy. He notes that the growth of social insurance and tax systems represent "built-in stabilizers" which allow policy makers to "smooth out" the peaks and valleys of business cycles. And through extensive labor market policies which include not only unemployment compensation but also subsidies to firms to retain and retrain workers who might otherwise be laid off, as well as through large increases in public employment, governments can maintain near-full employment in spite of the uncertainties of demand inherent in an open economy. In addition, through the provision of capital funds to the private sector (Lindbeck, 1974, pp. 9, 214–27), governments may be able to dampen the effect on capital accumulation of volatile profits in export industries as well as stimulate the development of import-substitution industries.[17]

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[16] Recent Swedish experience confirms the argument here. Capitalizing on the rise in prices in several of the nations to which it exports goods, Swedish exports enjoyed high profits in 1973–74. Subsequent wage negotiations in late 1974 provided unusually large increments. As a result, prices for Swedish goods rose in the world market and, even with slender profit margins, firms lost markets. The result has been large balance of trade deficits, three devaluations since 1976, and negative growth in 1977.

It may not always be the case that an expansion of the role of the state provides a sufficient means by which governments can enhance their control over open economies. Indeed, Vernon (1974) suggests that the efforts by European states to formally "plan" their economies failed because of increasing openness produced by the creation of the EEC and EFTA and the maturation of multinational enterprises: "None of these efforts to shore up the idea of the independent comprehensive national plan had much chance of succeeding; the contradiction between independent national plans of that sort and open national boundaries was simply too strong" (p. 16). In spite of this caveat, it is reasonable to expect that the arguments of Myrdal, Lehmbruch, and Lindbeck may be reflected in the experience of the 18 nations during the last two decades. Applying their arguments about the response of governments to a condition of external economic dependence, one might expect that the expansion of the public economy was most pronounced in nations in which the economy is relatively "open," in the sense of being exposed to the vagaries of the international economy. Using the ratio of imports and exports as a proportion of GDP as a measure of "openness," this argument would predict a strong positive association between the level, and the rate of increase, of "openness" and the expansion of the public economy.

Findings: Why the Public Economy Expanded in 1960–75

Table 1 presents the result of an analysis of the five plausible explanations of the expansion of the public economy. That analysis, based on data for the 18 nations, supports the following conclusions:

(a) Contrary to Wagner's "law," the rate of growth in the economic affluence of a nation does not contribute to the expansion of the public economy. Apparently citizens' demands for services and their willingness to accept higher levels of taxes, or both, are not income-elastic. Instead, the analysis supports Wildavsky's argument that the public economy grows, in relative terms, when economic growth is modest ($B = -.35$). In high-growth nations such as Japan and France, the demands of government for funds can be met with the "dividend" produced by applying a constant share to an expanding economic product. In low-growth nations, where governments do not enjoy that dividend, however, the almost inevitable increase in funding required for "uncontrollable" costs associated with mandated programs, as well as discretionary increments and new programs, absorbs a larger share of the economic product.

(b) Contrary to Downs and to Buchanan and Wagner, budgets do not expand most easily when taxes are concealed in a "fiscal illusion." A high and increasing reliance on "hidden" taxes exerted a significant dampening effect on the degree of expansion of the public economy, as indicated by the strong negative coefficients in Table 1. Instead, the public economy expanded most rapidly after 1960 in nations which relied heavily, and to an increasing degree, on wealth-elastic taxes, such as those on personal and corporate incomes.

(c) Contrary to the skeptics' view, politics is important in influencing the scope of the public economy. The partisanship of government is associated with the rate of expansion, and whether a nation's government was generally controlled by Social Democrats (and their leftist allies), or by nonleftist parties, provides a strong clue to the relative degree of change in the scope of the public economy ($B = .54$). Thus, nations such as Sweden, Norway, and Denmark, in which leftist parties tended, on average, to possess a majority of the government's electoral base, experienced increases in

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[17] More recently, Lindbeck (1977, pp. 13, 42) notes that the increased internationalization of the economy has caused an upsurge in international coordination of economic policy. Two recent examples are the development of protectionist limitations on imports and domestic capacity restrictions in the European steel industry (the Davignon plan), and the institution of annual economic summit conferences of western heads of state (Rambouillet, 1975; Puerto Rico, 1976; London, 1977; Bonn, 1978).

Table 1. The Expansion of the Public Economy: Economic, Fiscal, Political, Institutional, and International Explanations

	Level and Rate of Increase in Economic Output		Reliance on Indirect and Social Security Taxes		Partisanship of Government and the Frequency of Elections		Inter-Governmental Structure and Degree of Centralization		Openness of the Economy	
Variable	Regression Coefficient	Beta Coefficient	Regression Coefficient	Beta Coefficient	Regression Coefficient	Beta Coefficient	Regression Coefficient	Beta Coefficient	Regression Coefficient	Beta Coefficient
Governments' revenues as percent of GDP, 1960	0.28 (0.91)[a]	.25	0.42 (1.49)	.36	0.09 (0.34)	.08	0.52 (1.78)	.46	0.09 (0.45)	.08
GDP per capita, 1960	–0.002 (0.77)	–.22								
Average annual increase, 1960–75, in real GDP	–1.34 (1.16)	–.35								
Percent of all governments' revenues from indirect taxes and Social Security contributions, 1960			–0.15 (1.10)	–.27						
Increase in percent of all governments' revenues from indirect taxes and Social Security contributions, 1960–75			–0.17 (0.75)	–.18						
Percent of government's electoral base composed of Social Democratic or Labor parties, 1960–75					0.10 (2.15)	.54				
Number of national legislative elections, 1960–75					0.21 (0.29)	.07				
Federal structure of government							–2.56 (0.82)	–.23		
Percent of all governments' revenues received by central government, 1960							0.13 (0.92)	.28		
Exports and imports as a percent of GDP, 1960									0.19 (4.86)	.79
Increase in percent of GDP represented by exports and imports, 1960–75									0.00 (0.00)	.00
Coefficient of determination (R^2)	.19	(.02)[b]	.20	(.03)	.37	(.24)	.31	(.16)	.67	(.60)

[a] Parentheses contain the regression coefficient divided by its standard error.

public revenues which were much larger, as a proportion of GDP, than those in nations such as Japan, Italy, or France, where the Left either participated in government only as the minority partner of nonleftist parties or was excluded from government altogether.

The frequency of electoral competition displays a modest positive correlation with the increase in the public economy, indicating that competition may indeed exert an inflationary impact on budgets. However, after we control for the effects associated with the partisan composition of government, the impact of the frequency of electoral competition is negligible ($B = .07$).

(d) Contrary to our predictions, federalism tends to dampen the degree of expansion of the public economy and centralization tends to facilitate that expansion. Any inherent tendency for aggregate revenues to increase in federal systems because of the fragmentation of spending authority among several quasi-autonomous levels of government is apparently more than offset by other effects. Among these may be the larger number of access points which federalism provides for those who wish to intervene in public policy making in order to oppose the extension of government activity (Heidenheimer, 1975, pp. 20–29, 48–65). Also, the fragmentation of authority which characterizes federalism may contribute to an aggregate pattern of offsetting policy developments among the subnational units that lessens the magnitude of change in the nation taken as a whole. A high degree of centralization, on the other hand, seems to facilitate the expansion of the public economy ($B = .28$). Thus, it appears that that expansion was greatest in unitary, highly centralized nations. We might speculate that that institutional arrangement minimizes the effects of fragmentation and provides the means by which national elites can insure uniformity in existing policies and can most easily avoid the institutional obstacles to policy innovation.

(e) As the discussion of the concept of the open economy suggested, a high degree of trade dependence is conducive to a relatively large expansion of the public economy. Nations with open economies were far more likely to experience an increase in the scope of public funding than were nations with relatively closed economies ($B = .79$). Apparently, governments in nations with open economies have sought to counter the effects of external dependence by expanding their control over the domestic economy through the "nationalization" of a large portion of consumption (Lindbeck, 1974, p. 9).[18]

In order to ascertain the importance of each of the explanations presented in Table 1, I performed a regression analysis which included the six variables most closely associated with the expansion of the public economy. Table 2 presents the results of that analysis. The analysis suggests that about 75 percent of the variation among the nations in the degree of expansion can be accounted for by the six variables. The analysis also suggests that two variables are far more important to the explanation than the others; the two are the partisanship of government ($B = .34$) and the openness of the economy ($B = .58$).

Figure 1 arrays the measure of the expansion of the public economy with that for the partisanship of government. The array provides a clue as to why the strong bivariate relationship between the two ($r = .60$) is reduced in the regression analysis. It suggests that the dominance in government of leftist parties was a sufficient condition for a relatively large increase in the scope of public activity; there was no nation in which the Left had a large share of the government's electoral base which did not also experience a relatively large increase in the public economy. However, leftist domination was not a necessary condition, since several nations experienced large increases in spite of the absence of a strong leftist representation in government. Included in this latter group are the Netherlands, Belgium, Ireland, and Canada. All share at least one common trait: their economies are relatively open. To some extent, then, the impact of partisanship on the scope of the "tax state" is more pronounced in larger nations with more closed economies than in the smaller nations with more open economies. In the latter, apparently, all governments—whether formed by leftist or nonleftist parties—have been impelled by the exigencies of the open economy to expand the role of the state. To illustrate this, Table 3 presents the year-by-year change in the measure of the public economy in four nations in which the partisan control of government alternated between leftist and nonleftist parties. In the two larger nations, Britain and Germany, a clear partisan effect is noted.

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[18] Lindbeck notes here the distinction—relevant to Sweden and several other small open economies—between the nationalization of the consumption of income, which implies a large "tax state," and the nationalization of the production of income, which implies, instead, a large number of publicly owned enterprises.

In Britain, for example, the change was positive in every year in which the Labour party was in power (1964–70, 1974–75), and was negative in five of the seven years during which the Conservatives governed. Similarly, in Germany, the years of Christian Democratic government, particularly when Ludwig Erhard, the former minister of economics and proponent of the neoliberal "social market policy," was chancellor (1963–66), were marked by modest increases or decreases, while the period of Social Democratic control was marked by a cumulative increase in the scope of the public economy. In contrast, in Denmark and Norway, nonleftist governments were no less inclined than those dominated by the Social Democratic and the Labor parties to increase the public revenues at a rapid rate, relative to the rate of economic growth. Thus, while the size of the public economy grew considerably in Denmark during the Social Democratic minority government and subsequent "red coalition" of 1964–67, the largest increase occurred during the nonleftist coalition government of 1968–71. Likewise, in Norway, the bourgeois coalition which governed during 1966–71 and again during 1972–73 expanded the scope of state activity at a rate which in some years approached that achieved by Labor governments.

The regression analysis presented in Table 2 suggests that the openness of the economy is the best single predictor of the growth of public revenues relative to the economic product of a nation. In Figure 2, the measure of openness is displayed with that of the change in the scope of the public economy. A strong relationship is observed and no major exceptions appear to the pattern of covariation: larger nations with more closed economies experienced relatively modest increases in the scope of the public economy compared to the smaller nations with open economies.[19] Within the latter group, one finds some distinction between the Scandinavian nations, where the Left frequently governed, and Belgium and Ireland, where centrists or conservatives usually dominated government.

Table 2. The Relative Importance of the Economic, Fiscal, Political, Institutional, and International Explanations of the Expansion of the Public Economy

	Increase, 1960–75 All Governments' Revenues as a Percent of GDP		
	Simple Correlation	Regression Coefficient	Beta Coefficient[a]
Level of public economy, 1960 (governments' revenues as percent of GDP)	.35	0.11 (0.46)	0.10
Average annual percentage increase, 1960–75, in real GDP	–.31	0.06 (0.09)	0.02
Percent of governments' revenues obtained from indirect taxes and Social Security contributions, 1960	–.23	–0.06 (0.68)	–0.12
Percent of government's electoral base composed of Social Democratic or Labor parties, 1960–75	.60	0.07 (1.75)	0.34
Percent of all governments' revenues received by central government, 1960	.21	0.08 (0.97)	0.17
Exports and imports of goods and services as percent of GDP, 1960	.78	0.13 (3.22)	0.58

Coefficient of determination (R^2) = .75. $\bar{R}^2$ = .61.

[a] Beta coefficient is the standardized regression coefficient

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[19] As Dahl and Tufte (1973, p. 130) suggest, there is a strong inverse correlation between the population of a nation and the degree of economic openness (r = –.65).

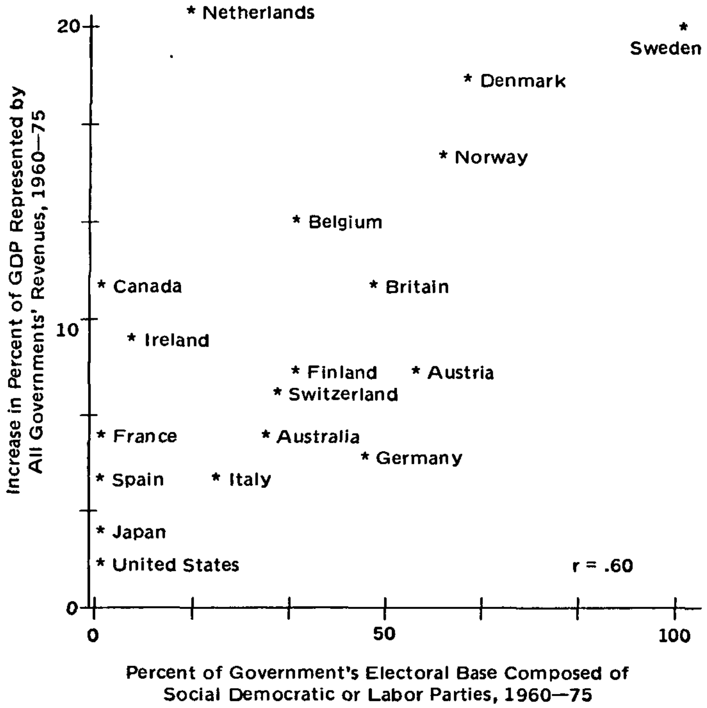

Figure 1. The Partisan Composition of Government and the Expansion of the Public Economy

Table 3. The Partisan Composition of Government and Change in the Scope of the Public Economy in Four Nations[a]

Year	Britain[b]	Germany	Denmark	Norway
1961	0.5	0.8	**–0.5**	**1.1**
1962	1.7	0.6	**1.5**	**1.4**
1963	–0.5	–0.1	**1.5**	**–0.1**
1964	–0.2	–0.4	**0.1**	**–0.5**
1965	**1.7**	–0.8	**1.3**	**1.2**
1966	**1.0**	0.5	**2.2**	1.1
1967	**1.8**	*0.6*	**0.6**	1.6
1968	**1.6**	*–0.5*	2.9	0.4
1969	**2.1**	*1.8*	0.2	2.1
1970	**1.3**	**–0.8**	4.5	0.5
1971	–1.9	**0.9**	3.4	**3.1**
1972	–1.7	**0.3**	**0.2**	**2.1**
1973	–0.8	**2.4**	**0.1**	**1.2**
1974	**3.3**	**0.3**	2.1	**–1.2**
1975	**0.8**	**–0.4**	**–3.1**	**1.5**

[a] Entries are the first-order changes between successive years in the percent of GDP received by all governments.

[b] Boldface entries indicate that the Social Democratic or Labor parties controlled government for at least six months during the year. The British entry for 1970 is credited to Labour, in spite of its defeat in the June election. Italicized entries for Germany denote the period of the Grand Coalition between the SPD and CDU/CSU (1966–69).

But the overall message is clear: the best explanation of why public authorities in some nations have expanded their control over the appropriation and allocation of resources while those in other nations have not is international in character. Among the nations considered here, the expansion of the public economy was most closely associated with a relatively high exposure to, and dependence upon, external producers and consumers.[20]

Discussion

The Domestic Consequences of the Open Economy. Why is the degree of trade dependen-

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[20] Contrary to our expectation, the extent of increase in openness during 1960–75 is not related to the extent of increase in the public economy. While all 18 nations became increasingly open during this period (in terms of exports and imports as a percent of GDP), the wide variation among the 18 nations was unaltered and the ordering of the nations remained, in 1975, almost as it was in 1960 (r = .95 between the measures of openness in 1960 and 1975). The findings reported here imply that it is the extent of openness, rather than the rate of change, that stimulates an expansion of the public economy.

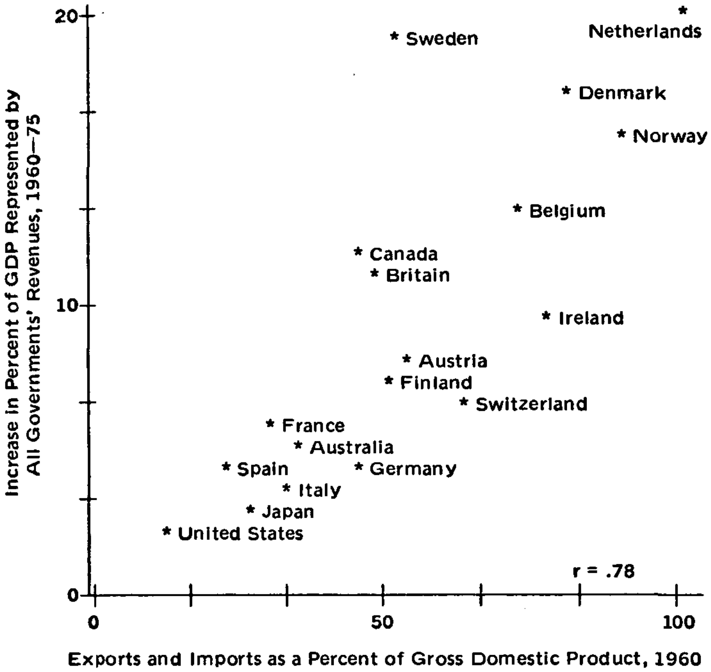

Figure 2. The Openness of the Economy and the Expansion of the Public Economy

ce the best predictor of the extent of expansion of the public economy? Is it simply the force of the exigencies posed by exposure to the international economy that causes government to extract and allocate a larger share of the economic product? Or does openness generate certain structural characteristics in advanced capitalist economies which are conducive to an expansion of the scope of the public economy? We shall attempt to provide an answer to these questions by identifying a sequence of economic, sociological, and political characteristics that derive, ultimately, from the openness of the economy. This sequence, presented in Figure 3, includes the following: (a) the degree of industrial concentration; (b) the density of unionization; (c) the scope of collective bargaining; and (d) the strength of labor confederations.

One of the structural attributes that frequently characterizes small, open economies is a high degree of industrial concentration—that is, an unusually large share of production and employment in a few large firms. According to Ingham (1974, pp. 40–41), "those societies in which industrialization was based upon exports because of too small a domestic market have tended to develop highly concentrated industries. Sharp competition in the export field has, in these countries, tended to force out the smaller and less-efficient companies which were less able to contend with fluctuations in world markets."

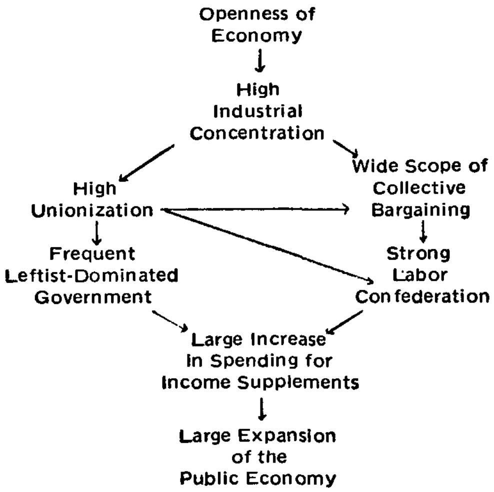
Openness of Economy
High Industrial Concentration
High Unionization
Wide Scope of Collective Bargaining
Frequent Leftist-Dominated Government
Strong Labor Confederation
Large Increase in Spending for Income Supplements
Large Expansion of the Public Economy

Figure 3. The Domestic Consequences of an Open Economy

High levels of industrial concentration appear, in turn, to facilitate the formation of employers' associations and labor confederations which include a relatively large portion of all firms and employees. Ingham notes, for example, that small, open, and highly concentrated economies tend to have "a small number of oligopolistic and non-competitive sectors . . . which facilitates collective organization" (p. 42). And the labor force in such economies tends to be somewhat less differentiated in terms of occupation and skill levels and, as a result, less fragmented (p. 43). The existence of such a labor force—relatively homogeneous and relatively concentrated in large firms in a small number of non-competitive sectors—is conducive to the growth of union organization. The existence of a relatively high level of unionization is, in turn, an important prerequisite for enduring leftist government, since unionized workers provide the core of the electoral base of most Social Democratic and Labor parties (see Stephens, 1978). Thus, in following the left-hand branch in Figure 3, we see that the openness of the economy contributes to an expansion of the public economy by facilitating the development of the social infrastructure upon which Social Democratic and Labor party electoral support rests.[21]

In addition to its effects on unionization and, ultimately, the structure of the party system, industrial concentration influences the scope of collective bargaining. The existence of "a small number of oligopolistic and non-competitive sectors," composed of a relatively small number of large firms, coupled with the proliferation of labor and employer organizations, widens the scope of collective bargaining. In some nations considered here, bargaining is decentralized and usually conducted at the level of the enterprise, either by enterprise unions as in Japan, or by national unions as in the United States. In most European nations, on the other hand, bargaining is conducted at the industry level with important additional negotiations at the enterprise level, as in Britain and Italy, or the regional level, as in Germany. But in the smaller open economies, industry bargaining often follows guidelines established in national negotiations (see Mouly, 1967; Schregle, 1974; Elvander, 1974; Lehmbruch, 1977; Hibbs, 1976). Thus, in Norway, Sweden, and Denmark, economy-wide "framework" agreements are formally negotiated between labor and employer confederations. And in Austria, Belgium, and the Netherlands, institutions exist in which representatives of the major confederations consult about, and occasionally negotiate, collective bargaining guidelines.[22]

One of the more important consequences of industrial and economy-wide collective bargaining is the power it bestows on labor confederations. Since they are the representatives of organized labor in economy-wide consultations and negotiations, they have tended to acquire formal powers over their affiliates in regard to collective bargaining. In Norway, Sweden, Finland, Austria, Belgium, and the Netherlands, for example, the major confederations can withhold strike funds from affiliates, thereby weakening the latter's ability to use the threat of strike action. In Austria, Norway, and the Netherlands, confederations can veto wage settlements obtained by their affiliates, and in these and most of the other open economies in Europe confederations have, *de jure* or *de facto,* the right to consult with affiliates prior to negotiations (Headey, 1970, pp. 421–25).[23]

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[21] This argument is supported by the existence of a positive correlation between the measure of the openness of the economy and the proportion of the work force that belong to labor unions (r = .41), and the positive correlation (r = .56) between the extent of unionization and the extent of government by leftist parties. Unfortunately, no satisfactory data exist with which cross-nationally comparable measures of concentration might be constructed. Data on unionization were obtained from Europa Publications (1977). We might note that our argument here implies a more complex relationship between the openness of the economy and the partisanship of government than is suggested by the assumption of independence in the regression analysis reported in Table 2. In fact, there is a positive, albeit modest, correlation between the two measures (r = .37).

[22] The argument in this paragraph is supported by the very strong correlation between the measure of openness and a measure of the scope of collective bargaining derived from Mouly (1967), Schregle (1974), Lehmbruch (1977), and Hibbs (1976). The correlation is r = .74.

[23] The argument that labor confederations are stronger, in terms of their ability to intervene in collective bargaining, in nations in which the scope of collective bargaining is relatively broad is supported by the strong correlation of r = .65 between the measure of the scope of collective bargaining (which ranges from 1 for Japan where collective bargaining often involves enterprise unions, to 6 for Austria, Ireland, Denmark, Norway, and Sweden, where economy-wide "framework" agreements, or similar consultations, take place) and a measure of the formal right of labor confederations to intervene in collective bargaining through prior consultation, post-negotiation approval, and/or control of strike funds.

The ability of labor confederations to intervene in collective bargaining makes them major actors in the political economy. This is especially true in nations where the domain of macroeconomic issues about which confederations consult with employers and/or government has been extended beyond collective bargaining in recent decades. This occurred in some nations through the creation of consultative institutions, such as the Austrian Parity Commission and the Dutch Foundation of Labor and the Social and Economic Council; in other nations, consultation is less institutionalized but equally important, as in Norway and Sweden where the confederation and the Labor party have traditionally represented the two arms of the labor movement (see Lehmbruch, 1977). This access to government can be used by labor confederations in many ways, but two are of special relevance here. Acting as representatives of the labor movement, confederations may advocate policies that will enhance the economic condition of their members by supporting programs that provide income supplements. On the other hand, confederations may act as allies of government—particularly when it is formed by Social Democratic or Labor parties—to moderate wage demands in the export sector in order to maintain international competitiveness. For example, they may voluntarily participate in programs of wage restraint negotiated between themselves, employer federations, and government (see Rall, 1975; Ulman and Flanagan, 1971; Galenson, 1973; Headey, 1970; and Lehmbruch, 1977). More frequently than not, labor confederations involved in such cooperative programs of wage restraint have felt compelled to use their access to government to obtain increments to the disposable income of their members, thereby compensating them for wage sacrifices. Whether they act as the representatives of organized labor or as allies of government, then, the existence of strong labor confederations tends to produce the same effect—an unusually large increase in publicly funded income supplements. And this, in turn, requires a comparable expansion in the scope of the public economy.[24]

The Consequences of the Expansion of the Public Economy. Does it matter that in some nations the public economy expanded to the range of 50 percent of GDP, while in others it increased only slightly in the years since 1960?[25] Thus far, this article has examined the reasons for the large cross-national variation in the degree of expansion of the public economy. It is appropriate to conclude, however, by shifting our attention from cause to consequence and examining the impact of changes in the scope of the public economy. A variety of effects might be considered, ranging from macroeconomic policy to electoral behavior; we have chosen two related effects which demonstrate the magnitude of the impact of different rates of cumulative change and the value dilemmas that accompany policy choices in this domain. The two involve economic equality and private capital accumulation.

In Figure 4, we illustrate the relationship among 12 of the 18 nations between the cumulative expansion of the public economy and measures of economic equality and private capital accumulation. A strong positive correlation (r = .83) exists between the size of the increase in the public economy and a measure of economic equality involving the difference in the proportion of all national income received, after taxes, by the top and bottom 20 percent of households. A strong negative correlation (r = −.89) exists between the size of the cumulative increase in the public economy and the change in the proportion of GDP represented by private capital accumulation.[26]

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[24] This argument is supported by the high correlation between the measure of the right of labor confederations to intervene in collective bargaining and a measure of the increase in the proportion of GNP spent on social security schemes (as defined by the International Labour Organisation) between 1960 and 1971. The correlation is r = .74. That increase in spending effort for social security schemes is, in turn, highly correlated with the measure of the expansion of the public economy (r = .72). The data on the increase in Social Security spending are reported in I.L.O. (1964, 1976).

[25] By 1975, the public economy absorbed 53.5 percent of the Dutch GDP, 50.2 percent of the Norwegian GDP, 52.2 percent of the Swedish GDP, and 45.0 percent of the Danish GDP. To a very large extent the scope of the public economy in 1975 was determined by the magnitude of the increase between 1960 and 1975. Thus the correlation between the measure of expansion in 1960–75 and the scope of the public economy in 1975 was r = .89, compared to a correlation of r = .35 between the 1960 and 1975 measures.

[26] The data on the size distribution of income are reported in O.E.C.D. (1976). The data used here represent the first-order difference between the proportion of all after-tax income received by the top 20 percent and the bottom 20 percent of households. To control for national differences in the size of households and enhance the cross-national comparability of the data, the O.E.C.D. reports standardized figures. These are the data used here. The measure of the

The data in Figure 4 suggest the existence of a tradeoff between relatively high degrees of economic equality and increasing rates of private capital accumulation.[27] And they suggest that the collective choice of a nation in regard to this tradeoff is strongly influenced by choices made in regard to taxation policy. Nations such as the United States, Japan, Spain, and Italy, where the extractive capacity of government did not significantly increase, relative to the economic product, have, in a sense, opted for a relatively inegalitarian distribution of income and an increasing rate of private capital accumulation.[28] Other nations, however, have, in the cumulative effect of their policies, made a different choice and have attained a relatively greater degree of economic equality at the cost of lower rates of private capital accumulation. Given the structural features that tend to accompany economic openness—a high degree of unionization, relatively frequent government by Social Democratic and Labor parties, strong labor confederations, and, ultimately, a large increase in taxation—it is perhaps not surprising that the nations which tended to favor distributional equity rather than private accumulation were those with open economies.

Conclusion

Most studies of public policy in advanced industrial society confine themselves to an analysis of internal, or domestic, causes and consequences. Only rarely, and only recently, have students of public policy examined the linkage between the international economy and domestic policy (Katzenstein, 1976, 1978). The predominant image implicit in most policy studies is that of political autarky—of autonomous states whose policy processes are wholly insulated from external influences. Yet in a world marked by "complex interdependence" (Keohane and Nye, 1977) such an image is increasingly anachronistic; because, as Cooper argues, "increased economic interdependence . . . erodes the effectiveness of national economic policies and hence threatens national

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change in the proportion of GDP that represents all private capital accumulation (i.e., savings and consumption of fixed capital) is the first-order difference of the proportions in 1960 and 1975. The data are reported in O.E.C.D. (1973, 1977a).

[27] That a tradeoff exists between equality and accumulation is further suggested by the high negative correlation between the two measures (r = −.74).

[28] In spite of the increase in the rate of capital accumulation in the private sector, the low rate of governmental savings—and indeed aggregate government deficits in several years—caused the proportion of gross capital accumulation in GDP to drop in Italy and the United States between 1960 and 1975. See O.E.C.D. (1977a).

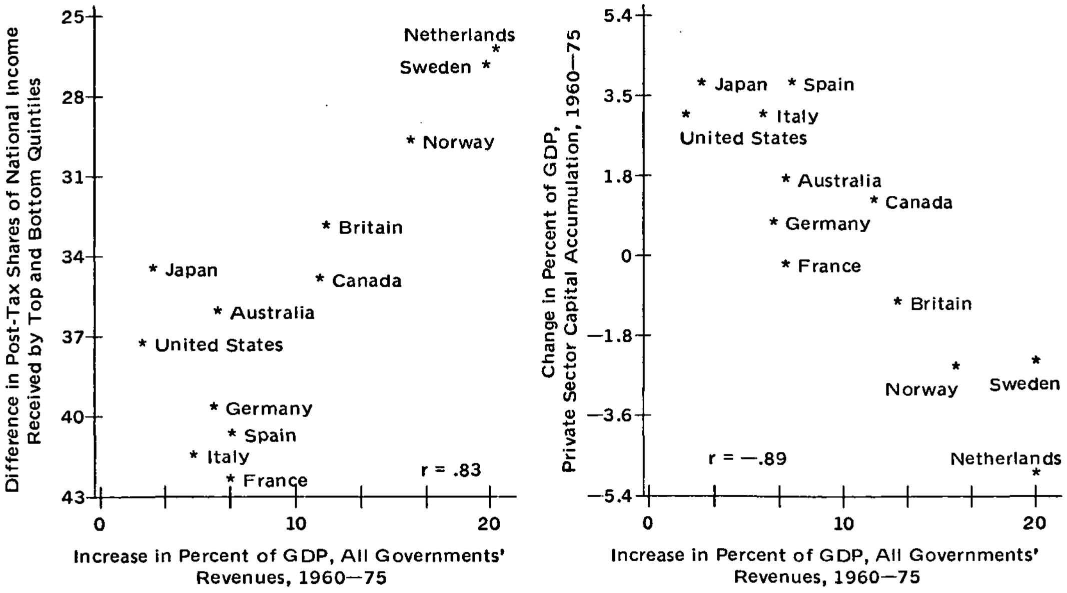

Figure 4. The Tax Tradeoff:
The Expansion of the Public Economy, Economic Equality, and Capital Accumulation

autonomy in the determination and pursuit of economic objectives," governing elites are likely to feel compelled to use public policy to confront the challenges posed by the international economy (1972, p. 164).

Governments use a variety of policy instruments to shelter their economies from the competitive risks of the international economy. Some states adopt explicitly neo-mercantilist policies (Katzenstein, 1978, pp. 879–920); others favor certain enterprises, whether in the private or public sector, as "national champions" (Vernon, 1974, Ch. 1); still others adopt a variety of industry-specific protectionist measures. Each type of policy has occurred in one or more of the nations considered here during the past decade. However, neo-mercantilism, support of "national champions," and protectionism are often ineffective for nations with open economies, given their small size relative to some of their most important trading partners. For such nations, another type of response is more feasible—one which is more defensive in character and involves a relatively large public economy. Governments in small open economies have tended to provide a variety of income supplements in the form of social security schemes, health insurance, unemployment benefits, job training, employment subsidies to firms, and even investment capital. Prompted in part by the incentive to maintain price competitiveness of export goods in the world market and accentuated by the social structural features generated by economic openness, this expansion of the role of government in the distribution and consumption of national income has dramatically enlarged the scope of the "tax state" in contemporary advanced capitalist society.

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The Predictability of Coups d'état: A Model with African Data*

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This paper specifies and estimates a model of the structural determinants of coups d'état for the new states of black Africa in the years from 1960 through 1975. Results indicate that (1) both social mobilization and the presence of a dominant ethnic group are destabilizing (these effects are additive); (2) multipartyism is destabilizing while electoral turnout in the last election before independence is stabilizing; (3) multipartyism is particularly destabilizing where a dominant ethnic group exists; (4) the presence of such a group reduces (but does not eliminate) the stabilizing effect of turnout; and (5) multipartyism has no pronounced effect on elite instability where turnout is high. Taken together, these patterns account for over four-fifths of the variance in coups d'état in black Africa in the period.

Over the past two decades, coups d'état have become increasingly common to the politics of the Third World generally and to African politics in particular. Their occurrence has attracted a fairly large body of literature that seeks to identify the structural determinants of coups and elite instability. Three broad determinants have received particular attention: first, social mobilization or "modernization" (e.g., Deutsch, 1961); second, cultural pluralism (e.g., Geertz, 1963; Kuper and Smith, 1969); and third, two "political" factors—political party systems and mass participation (e.g., Deutsch, 1961; Huntington, 1968). While it would be foolhardy to claim that there is a consensus on the ways in which these factors affect elite instability, the theoretical literature does suggest that coups d'état should be predictable.

Unfortunately, the opposite impression seems to emerge from empirical studies, especially those dealing with Africa.[1] A growing number of analysts have therefore concluded either that the wrong explanatory variables have been isolated, or (more radically) that the incidence of coups is not meaningfully dependent on *any* structural characteristics. For example, Zolberg wrote in 1968 that "it is impossible to specify as a class countries where coups have occurred from others which have so far been spared" (1968, p. 71). More recently, Decalo has suggested that the effects of the "structural characteristics of political systems" on elite instability are trivial when compared to the "idiosyncratic element" (1976, p. 22). Such statements clearly imply that the incidence of coups d'état is random with respect to political and social structure, and that such events are therefore unpredictable.

The purpose of this article is to show that this conclusion is unnecessarily pessimistic, and that the emphases in the theoretical literature are not misplaced. More specifically, social mobilization, cultural pluralism, and the two political factors identified above *are* important structural determinants of elite instability. While analysts have differed over the exact nature of the effects of these factors, such differences can be resolved empirically. Once this is done, the incidence of coups d'état can be shown to exhibit a considerable degree of predictability.

The Effects of Social Mobilization. According to Deutsch (1961, pp. 497–98), social mobilization is a politically important process because it expands "the politically relevant strata of the population," by which is meant "those persons who must be taken into account in politics" (Deutsch, 1961, pp. 497–98). This has led some (e.g., Finer, 1962, pp. 87–88; Fossum, 1967, p. 235) to argue that increased mobilization is likely to decrease the probability of

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[1] Recent empirical studies include those by Morrison and Stevenson (1972), Wells (1974), and Barrows (1976). Of these analysts, only Wells provides evidence on the fit of his model (in the form of multiple correlation coefficients), but, as Phillips and Bar-Yunus (1976) have pointed out, his models actually account for between zero and 12 percent of the variance in coups e'état. Indirect evidence in the other two papers suggests a similar degree of fit.

coups, since these are covert events instigated by small numbers of people. If the number of participants in a mobilizing polity is rising, it should become increasingly difficult for such small factions to mount coups.

An alternative argument identifies other factors that may condition or even reverse the basic effect. The most prominent of these has been labeled variously as political "performance" and "capacity" (Deutsch, 1961) or political "institutionalization" (i.e., the process by which organizations "acquire value and stability" [Huntington, 1968, pp. 12–24]). If mobilization is not accompanied by an increase in government capacity, governments are unlikely to respond to and satisfy the new aspirations and expectations that are generated by social mobilization. Under such circumstances, the effect of mobilization on instability is reversed and becomes *positive.* This clearly constitutes a major change to the basic argument (Deutsch, 1961, p. 502; Huntington, 1968, Ch. 1, especially p. 55).

This second hypothesis about the effects of social mobilization is of particular interest in the African context, because Huntington (1968, p. 13) and others have suggested organizational age as one key feature of institutionalization. Perhaps the most striking feature of the black African states is their youth, which, along with the rapidity of the decolonization process, suggests that national political institutions in Africa are unlikely to meet Huntington's initial criterion for institutionalization. If this is the case, we might simply expect social mobilization to produce more coups d'état in Africa.

The Effects of Cultural Pluralism. A number of analysts have suggested that cultural pluralism fosters political instability. By cultural pluralism, I refer to societies that are heterogeneous with respect to ascriptive or "primordial" attachments; specifically, attachments and identifications based on linguistic, religious, or ethnic ties (c.f., Shils, 1957; Geertz, 1963, p. 103; and the essays in Kuper and Smith, 1969). Typically, social attachments based on these criteria tend to reinforce one another. It is commonly argued that such primordial ties are important because they compete with and often predate attachments to the civic state itself, thereby forming the basis of political conflict (c.f., Deutsch, 1957, p. 62). Initially, therefore, it is reasonable to argue that cultural heterogeneity is destabilizing in and of itself (c.f., Rabushka and Shepsle, 1972).

Cultural pluralism is usually hypothesized to be particularly destabilizing as societies experience social mobilization, since mobilization makes different elements within the population increasingly aware of group differences, which in turn reinforces the salience of the primordial ties themselves (Deutsch, 1961, p. 501). In contrast, culturally homogeneous societies are not subject to the same pressures during the process of social mobilization, given the absence within them of primordial attachments that compete with symbols of the state.

There is, however, a minority view that predicts just the opposite effect. Coleman (1960, p. 368) suggests that by providing a system of countervailing power centers, cultural pluralism may contribute to political stability, *provided that no one group is in a numerically dominant position* (see also Dahl, 1956, p. 17). The potential significance of this argument in the African context is particularly clear when one recalls that, as a group, African states are characterized by a considerable degree of cultural heterogeneity (compare the data on ethnicity in Morrison, et al., 1972, Pt. 2).

In short, there are three distinct ways that cultural pluralism may affect elite instability. The dominant argument is that pluralism is destabilizing, which implies a simple positive effect. In addition, Deutsch's argument about the joint effects of mobilization and pluralism implies a nonadditive model, where the magnitude of the positive effect of mobilization increases with cultural heterogeneity. These two arguments are, however, incompatible with the third (countervailing-power) hypothesis that predicts a simple *negative* effect of pluralism on instability, on the grounds that the presence of many groups precludes political violence in the form of coups by any one group.

The Effects of Party Systems and Mass Participation. It is commonly argued that multipartyism and mass political participation are both destabilizing. Huntington (1968, pp. 425–26), Weiner and LaPalombara (1966, p. 416), and others claim that multipartyism generally gives expression to and thus exacerbates pre-existing social cleavages. In contrast, systems with one (or perhaps two) parties are seen as more stable to the extent that these dominant parties can aggregate competing political interests, thereby transcending those otherwise destabilizing social cleavages.

It is usually also claimed that the effects of multipartyism depend on the level of mass political participation. In particular, Huntington and others argue not only that participation is in itself destabilizing, but further that participation in multiparty systems is especially destabilizing because the interests that are thus aroused are unlikely to be satisfied. This implies that these two variables have joint effects, such that the probability of coups increases dramatically with participation in multiparty systems. An alternative but less popular view is that mass participation is in fact *stabilizing:* by increasing the number of politically relevant persons it decreases the opportunities for small factions to engage in such clandestine political activities as coups d'état (c.f., Deutsch, 1961; Hayward, 1973). This suggests different estimates for the joint-effects model, where high rates of participation combined with a dominant political party reduce the probability of elite instability.

In addition, if multipartyism does exacerbate social cleavages, we might expect joint effects between the party-system variables and cultural pluralism. For example, the analyses I have reviewed suggest that a strong one-party system may help reduce any destabilizing effects of ethnicity. Similarly, mass political participation may condition any such effects of ethnicity. Finally, a slightly more complex possibility is that ethnicity *and* one-party dominance *and* mass participation may have a unique combined effect on elite instability, the specific nature of this effect depending in part on whether cultural pluralism and mass participation are stabilizing or destabilizing.

Analysis

The Dependent Variable. The analysis centers on those successful or attempted "irregular" government changes that are more commonly known as coups d'état. These changes are irregular in that they are attempts by insurgent elites to remove ruling regimes from power by extraconstitutional means, and are accompanied by actual or threatened resorts to physical violence. That is, coups are relatively covert actions that ignore or bypass the regular channels or "rules of the game" concerning the succession process.[2]

The dependent variable itself consists of a count over the period from 1960 (or from the year of independence, if that came after 1960) through 1975 of all successful, unsuccessful, and plotted coups d'état (typically, although not invariably, initiated by the military). I have followed the definitions and coding procedures for the three forms of coups that are described in some detail by Morrison, et al. (1972, p. 128). First, a *successful coup* is an event in which the existing political regime is suddenly and illegally displaced by an insurgent elite group without overt mass participation in the event itself. To be coded as successful, such displacements have to last at least one week. Second, an *unsuccessful coup* is an event where *a physical attempt is made* to displace the regime, but the attempt fails in the sense that the displacement lasts less than one week. Finally, *plotted coups* are defined as "events in which an announcement or admission is made by the elite group in power that a plot to overthrow the government by violence has been discovered" (Morrison, et al., 1972, p. 128). Note the important distinction between plots and unsuccessful coups: the latter require the existence of a physically attempted take-over of government.

New data on these three forms of coups were collected for the 30 countries listed in the appendix (all of these countries were independent by 1965). These data cover the years from independence or 1960 (whichever came later) through 1975, inclusive. Source coverage was wide, and included the following: *Keesing's Contemporary Archives, Facts on File, New York Times, Africa Recorder, Africa Diary,* and the *African Digest.* Following the analysis of Morrison and Stevenson (1971), I combined the three events into an additive index, with successful coups weighted by 5, unsuccessful coups weighted by a factor of 3, and plots weighted by 1. This procedure is designed to weight the different events by an estimate of their political significance.[3]

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[2] Note that a concern with the problem of leadership succession is central to the literature on political development. See, for example, Weiner and LaPalombara (1966) and Huntington (1968). In this paper, I use the terms coups d'état and political or elite instability interchangeably, although I recognize that political instability can manifest itself in forms other than the coup d'état.

[3] Morrison and Stevenson (1971, pp. 361–64) provide an analysis of the dimensional properties of their similarly weighted index. I follow their weighting since it results in smaller residual sums of squares in the models to follow than do a series of plausible alternative weights (see Tufte, 1969 for a discussion of this procedure). For the 30 countries listed in the Appendix, values on my measure range from zero to 46 (Dahomey [Benin]), with a mean of 13.967 and a standard deviation of 12.294. Thus, this variable is somewhat skewed, although not substantially so (since the mean is larger than the standard deviation). Country values are plotted on the vertical axis of Figure 1 below. For a comparison of the different sources used in the collection of my data on coups, see Jackman and Boyd (1979).

The reliability of my measure of elite instability can be estimated by its covariance with the Morrison and Stevenson Index (MSI), because each was coded independently of the other. For the purposes of this comparison, I omit events from my index that occurred after 1969, since the MSI covers the period from independence to 1969. The product-moment correlation between the two variables is .95, which is clearly an acceptable reliability figure. In addition to this correlation coefficient, a regression of my instability index (from 1960 to 1969) on the MSI should, if the measures are identical, produce an intercept of zero and a slope of one. For the 30 countries I estimate an intercept of .65 (standard error: .81) and a slope of 1.16 (standard error: .07). These figures are very close to zero and unity respectively. To the extent that there is a discrepancy, the scores on my measure are slightly higher than the MSI scores. This I attribute to my use of *Keesing's Contemporary Archives*, which reported some events not covered in the other sources. The remainder of the analysis is based on my instability index using data that cover the full period from c. 1960 through 1975.

Mobilization and Cultural Pluralism. I start by examining the effects of social mobilization and cultural pluralism on coups d'état. Deutsch (1961) has provided a number of operational indicators of social mobilization, including the size of the nonrural population, the size of the nonagricultural work force, the size of the mass media audience, and literacy rates. In the following analysis, social mobilization is measured by the simple sum of the percentage of the labor force in nonagricultural occupations (c. 1966) and the estimated percentage of the population that is literate (c. 1965).[4] This measure reflects the key elements of Deutsch's discussion. First, and most important, it reflects variations in the importance of the market economy, since in the African case, "agriculture" includes a substantial subsistence component. Second, it partly reflects the size of the nonrural population, for obvious reasons. Finally, it also taps exposure to "modernity," in the sense of technology, non-subsistence economic values, education, and susceptibility to mass communications.

Defining and measuring cultural or ethnic pluralism is a somewhat more challenging task, since there are at least ten distinct criteria available for defining ethnic units (Naroll, 1970, p. 726). Here I follow the definition proposed by Morrison, et al. (1972, pp. 166–70, 418), who require that each ethnic unit consist of persons who share basic cultural characteristics—that is, who share patterns of marriage, descent, community organization, authority, and economic structure. My measure of cultural pluralism is taken from the country profiles in Morrison, et al., and consists of the percentage of the population in the largest ethnic group. This variable is intended to measure the potential for political dominance that any one group has simply on the basis of its numerical strength. Not surprisingly, it is highly (negatively) correlated with the total number of ethnic groups in the society.[5] Thus, low scores on this variable represent a fragmented situation of the type described by Coleman (1960), Deutsch (1961), and others.

[4] Data on the agricultural component of the labor force are from the United States Agency for International Development (1968), and are the same as those reported two years later in the same publication and those listed by Morrison, et al. (1972, p. 40). I have reversed the AID scores so that this variable reflects the percentage of the labor force in nonagricultural (rather than agricultural) occupations. For the 30 countries in this study, values on this variable range from 4 to 44, with a mean of 16.333 and a standard deviation of 9.320. Data on literacy rates are from Morrison, et al. (1972, p. 70), and values on this component of social mobilization range from 3 to 40, with a mean of 14.533 and a standard deviation of 10.692 ($N = 30$). For the same countries, values on the social mobilization index range from 7 to 74, with a mean of 30.867 and a standard deviation of 17.087.

[5] I have treated the "other" category as a separate (and single) ethnic group. C (the percentage of the population in the largest group) ranges from 19 to 95, with a mean of 46.017 and a standard deviation of 19.292. The correlation of this variable with the total number of ethnic groups is –.794 (this second variable ranges from 2 to 9). In general, the relative size of ethnic groups is a variable that exhibits substantial temporal stability (the political and social relevance of ethnicity to individuals may, of course, be much less stable).

While it is a simple measure, I use the percentage of the population in the largest group instead of the more common measure of fragmentation (F) proposed by Greenberg (1956) and Rae and Taylor (1970), since in its middle ranges the latter is insensitive to what I think are important differences. Consider two countries, the first with three groups of equal size (34, 33, and 33 percent each), and the second with four groups (50, 25, 15, and 10 percent of the population, respectively). The F scores for these two cases are so similar (.667 and .655) that they obscure the important fact that in the first case no group is dominant, while in the second the largest group is twice the size of the next largest (and five times greater than the smallest).

Moderate to high scores, on the other hand, indicate the presence of a potentially tyrannical majority (Dahl, 1956, p. 17), along with a more culturally homogeneous population. Note, however, that none of the societies in this analysis is completely homogeneous, in that none has fewer than two ethnic groups.[6]

To gauge the effects of social mobilization and cultural pluralism on instability, I focus on the following model:

$$Coups = \alpha + \beta_1 M + \beta_2 C + \beta_3 (M.C) + \epsilon \qquad (1)$$

where M is Social Mobilization, and C is Cultural Pluralism. The earlier discussion implies $\beta_1 > 0$, but provides contradictory expectations concerning β_2 and β_3, which incorporate the effects of ethnic dominance. If, as most analysts argue, ethnic heterogeneity is destabilizing, then we would expect $\beta_2 < 0$, while $\beta_2 > 0$ would be consistent with the countervailing-powers hypothesis that sees ethnic dominance as the destabilizing force. Note that joint effects are also specified in this equation (estimated by β_3) that allow the effects of mobilization to vary according to the degree of cultural pluralism. This is clearly necessary if we are to assess the validity of the arguments of Deutsch and others that were reviewed earlier.[7]

The top two rows of Table 1 report the least-squares estimates for two versions of equation (1), one with additive effects only (i.e., with β_3 constrained to a value of zero) and the other with both additive and multiplicative terms. It is clear that mobilization and ethnicity do not have nonadditive (or joint) effects on coups. This result is obvious from the fact that the estimate of β_3 is not effectively different from zero (the t-ratio is 1.29).[8] Moreover, a

Table 1. Regressions of Coups d'état, 1960 to 1975, on Social Mobilization and Cultural Pluralism (N = 30)[a]

	Constant	Social Mobilization (M)	Size of Largest Ethnic Group (C)	$M.C$	R^2	$\bar{R}^2$
Continuous Version of Ethnicity	−6.137 (8.065)	.375 (.131)	.019 (.012)		.241	.184
	8.775 (13.867)	−.354 (.570)	−.016 (.028)	.0018 (.0014)	.288	.206
Binary Variable Version of Ethnicity[b]	−3.609 (4.666)	.368 (.111)	11.651 (3.726)		.390	.344
	−3.886 (6.535)	.376 (.172)	12.095 (8.132)	−.014 (.228)	.390	.319

[a] Main table entries are the parameter estimates, and numbers below them in parentheses are their standard errors.

[b] This variable equals 1 if the dominant ethnic group constitutes at least 44 percent of the total national population, and zero otherwise.

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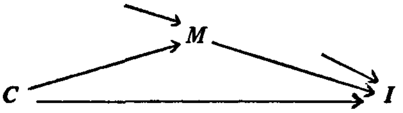
[6] Space considerations preclude a full treatment of the definition and measurement of ethnicity here, but Morrison, et al. (1972, pp. 166–70, 415–33) provide a lengthy discussion of the term and of issues surrounding the validity and reliability of the data used below.

[7] Although this point is straightforward, it seems to have been overlooked in previous tests of this hypothesis. For example, Morrison and Stevenson (1972, p. 101) specify the following model (using the notation of my equation [1]):



They then argue that if "modernization *interacts* with cultural pluralism to increase the likelihood of political instability" (their emphasis), P_{MC} and P_{IM} will have opposite signs. This is a clear misspecification, since by allowing only for additive effects, their model precludes the possibility of an interaction between M and C. The same problem exists with Barrows' (1976, p. 175) analysis, which is based on partial correlations from an additive model only.

[8] Statistical tests of significance help distinguish estimates based on these data from estimates derived from a randomly generated set of data. The precision of an estimate increases with its t-ratio (i.e., the ratio of an estimate to its standard error). A t-ratio of 2.0

comparison of the corrected coefficients of determination shows that the full equation accounts for no more variance than does the truncated version with additive effects only. We can therefore conclude initially that the effect of mobilization on coups does not depend on the degree of cultural heterogeneity, which means that we can confine our attention to the estimates for the additive model in the top row of the table.

The estimate for β_1 in the additive model is positive and almost three times the size of its standard error. That is, mobilization increases the probability of coups d'état, as was anticipated. Such an outcome is consistent with the arguments put forward by Deutsch and Huntington, among others, that in the absence of political capacity or institutionalization, mobilization is likely to be destabilizing. Remember that this interpretation of the estimate for β_1 is predicated on the assumption that both the recency of national independence and the decolonization process imply a generally low degree of institutional longevity in black Africa.

Finally, the estimate for β_2 in the additive model is also positive and larger than (although not twice as large as) its standard error. This means that we can reject the widely held position that cultural heterogeneity is *de*stabilizing. In fact, this estimate suggests that the probability of coups increases with the size of the largest ethnic group, which is consistent with the countervailing-powers hypothesis that it is ethnic *dominance* rather than ethnic heterogeneity that has destabilizing effects.

Having secured these basic estimates for equation (1), I examined scatterplots to see whether the assumption of linearity is unnecessarily strong. This process, along with estimates of equation (1) based on feasible nonlinear transformations of the independent variables, indicates that the effects of mobilization (M) are best treated as linear. In contrast, as is shown in the bottom two rows of Table 1, the effects of ethnic dominance (C) are best captured when a binary variable is created from that measure. The optimal fit was secured when size of the largest ethnic group was dichotomized just below the median, in the sense that this classification produces smaller residual sums of squares than others, and it also leaves two categories of equivalent size. I therefore created a binary variable equal to one if the largest group constituted at least 44 percent of the population ($N = 16$), and zero otherwise ($N = 14$).

As is clear from the bottom panel of Table 1, the revised estimates still provide no support for the nonadditive argument (the joint effects of mobilization and cultural pluralism [β_3] do not differ meaningfully from zero). The revised estimates for the additive model do, however, produce a better fit (the $\overline{R}^2$ of .344 is increased by almost 90 percent over the earlier figure of .184), while the impact of social mobilization is similar in both versions of the additive model (the t-ratio for the revised estimate of β_1 is slightly larger).

The most important difference between the two versions of the additive model in Table 1 is that recasting C in the form of a binary variable yields a much more precise estimate of β_2, which is over three times the size of its standard error. This indicates that the impact of ethnic dominance on coups is best treated as nonlinear.[9] More specifically, the critical difference appears to lie between those countries where the size of the largest ethnic group falls just short of a majority (44 percent), and those where the largest ethnic group is smaller than this. The estimated value of 11.65 for β_2 indicates that this difference accounts for almost one standard deviation change in the dependent variable. Thus, it is the presence of a numerically dominant ethnic group (rather than cultural heterogeneity) that appears to be the potent destabilizing force. This, of course, is consistent with the countervailing-powers hypothesis.

Political Party Systems. Earlier, I distinguished between two features of party systems. The first of these reflects the number and relative size of the parties and can range from one-party systems to multipartyism: I shall call this party dominance. The second characteristic has to do with rates of political participation in conventional (i.e., electoral) politics. I shall deal with these in turn.

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indicates that the results are significant at the .05 level, while a t-ratio of 1.7 indicates significance at the .10 level. Note that standard errors of estimate are sensitive to the number of observations, which in the present case is small (30).

[9] It is also possible that the binary variable version of ethnic dominance provides a better fit because it is more reliable than the continuous version. In particular, the dichotomous version of this variable is probably less sensitive to the unreliability that arises from the more detailed reliance on the various ethnographies required to construct the continuous variable (c.f., Morrison, et al., 1972, p. 169).

To measure party dominance, I rely on the percentage of the vote cast for the winning party *in the election closest but prior to the date of independence.* It is important to emphasize that the date for this variable *precedes* any of the instability events counted in the dependent variable. Thus, there is no ambiguity about the causal ordering when this variable is treated as an explanatory variable. Instead, by identifying the strength of political parties at independence, this variable isolates variations in an *initial* structural condition that we expect on theoretical grounds to have an impact on subsequent levels of elite instability. The data themselves are again taken from Morrison, et al. (1972, p. 103).[10]

To measure political participation, I have taken data on electoral turnout from the same source (p. 102). These data apply to the same election as the party dominance scores (i.e., the election closest but prior to independence), so they, too, identify an initial political condition. This variable expresses the number of voters participating in national legislative elections as a percentage of the population.[11] Note, incidentally, that the zero-order correlation between party dominance and participation is only .126.

The following model estimates the effects of party dominance and participation on coups d'état:

$$Coups = \alpha + \beta_4 D + \beta_5 P + \beta_6 (D.P) + \epsilon \qquad (2)$$

where D is party dominance, and P is political participation. The arguments we have reviewed see multipartyism as destabilizing and party dominance as stabilizing, which implies $\beta_4 < 0$. Because there is disagreement in the literature over whether mass participation is destabilizing, we have no specific expectations concerning the sign of β_5. The last parameter (β_6) is intended to estimate the magnitude of any joint or nonadditive effects of party dominance and participation on coups.

Table 2 reports the estimates for two versions of equation (2). Examining the top two rows, notice first that the nonadditive model fits the data much better than does the additive version. Therefore, the effects of party dominance and voter turnout are mutually dependent. In fact, the nonadditive model also provides a much more precise estimate of β_5 than does the simpler equation (the t-ratio increases from 1.2 to 2.0). Moreover, the estimate in the second row for β_4 is negative, as predicted. This provides strong support for the arguments of Huntington and others that fractionalized party systems have destabilizing con-

Table 2. Regressions of Coups d'état, 1960 to 1975, on Party Strength and Participation at Time of Independence (N = 29)[a]

	Constant	Percent Vote to Winning Party (D)	Electoral Turnout (P)	$D.P$	R^2	$\bar{R}^2$
Continuous Version of Participation	40.784 (6.670)	–.334 (.086)	–.199 (.161)		.409	.364
	64.904 (15.284)	–.646 (.198)	–1.345 (.676)	.015 (.008)	.473	.410
Binary Variable Version of Participation[b]	37.910 (5.970)	–.315 (.088)	–5.528 (3.793)		.421	.377
	46.008 (6.692)	–.448 (.102)	–31.091 (12.200)	.375 (.171)	.514	.456

[a] Ethiopia is excluded from this analysis. Main table entries are the parameter estimates, and numbers below them in parentheses are their standard errors.

[b] This variable equals 1 if turnout was 21 percent or more, and zero otherwise.

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[10] This variable ranges from 16 to 100, with a mean of 66.724 and a standard deviation of 22.000 (N = 29). I have modified two of the original scores. First, since Ethiopia has no political parties (and given also that it was an "ancient kingdom" in 1960), I have excluded it from further analysis, which reduces the N to 29. Second, I have given the Sudan a score of 63 percent, on the grounds that even though Morrison and his colleagues do not use this value, they do suggest this as the best estimate (since the National Unionist Party won 60 percent of the seats). For discussion of the reliability of this and the following measure, see Morrison, et al. (1972, pp. 96–97).

[11] This variable ranges from 1 to 46, with a mean of 22.483 and a standard deviation of 11.825 (N = 29).

sequences. Finally, the estimate of β_5 is also negative, which is consistent with the argument that higher participation rates foster stability.

There is, however, a problem with the estimates in the second row of this table. Specifically, the estimate for α (64.904) is too high, since it is 41 percent higher than the observed maximum value of the dependent variable (46). To correct this problem, and to check on the assumption of linearity for β_3 and β_4, I examined the scatterplots and tried alternative nonlinear transformations on these independent variables. These procedures indicate that *D* (the largest party's share of the vote) is best treated as linear, while *P* (electoral participation) is best treated as a binary variable that assumes a value of one if turnout was more than 20 percent ($N = 15$), and zero otherwise ($N = 14$).

Revised estimates of equation (2) using the analysis of covariance model implied by this binary variable treatment are shown in the bottom two rows of Table 2. The superiority of the binary variable version of participation is reflected in the higher *t*-ratios for β_5 and β_6 in the bottom row, in the higher multiple coefficient of determination, and in the more reasonable estimate for α. These revised estimates also highlight the superiority of the nonadditive formulation over its simpler additive counterpart.

Substantively, the estimates in the bottom row of Table 2 show that within high-turnout countries the stabilizing effects of party dominance are negligible, and that coups are particularly likely where turnout is low *and* the party system is highly fractionalized. These results are consistent with the general emphasis in the literature on the destabilizing effects of multipartyism. However, they also suggest that the common "participation is destabilizing" argument is misplaced. Besides showing that participation has a stabilizing impact, the estimates imply that the stabilizing roles of participation and party dominance may be somewhat interchangeable.

Mobilization, Pluralism, Party Dominance and Turnout. The analysis so far indicates that both social mobilization and cultural pluralism have additive effects on coups d'état. It also points to a more complex nonadditive effect of party dominance and political participation. Yet these conclusions are provisional in that they do not address the issue of whether mobilization and pluralism have effects *net* of the two party-systems variables, and vice versa. Nor have we yet pursued the possibility that ethnic pluralism may condition the effects of the two "political" variables.

To answer these questions, I specify the following more complete model that includes components from equations (1) and (2):

$$\begin{aligned} Coups = \alpha + \beta_1 M + \beta_2 C - \beta_3 D - \beta_4 P + \\ \beta_5 (D.P) + \beta_6 (C.D) + \beta_7 (C.P) + \\ \beta_8 (C.D.P) + \epsilon \qquad (3) \end{aligned}$$

where all terms are defined as before. That is, *Coups* are counted from 1960 to 1975; *M* is Social Mobilization (the sum of the percentage of the labor force in nonagricultural occupations, c. 1966, and the percentage of the population that is literate, c. 1965); *C* is a binary variable that equals one when the largest ethnic group comprises at least 44 percent of the population, and zero otherwise; *D* is Party Dominance at independence (in terms of the percentage of the vote won by the largest party); and *P* is a binary variable that equals one when turnout in the last election before independence was more than 20 percent of the population, and zero otherwise.

This model specifies coups as a positive function of both mobilization and ethnic dominance (in light of the results of the first section we expect $\beta_1 > 0$ and $\beta_2 > 0$). Consistent with the results of the last section, it also specifies instability as a negative function of both party dominance and participation, so that $\beta_3 < 0$ and $\beta_4 < 0$, while $\beta_5 > 0$. In addition to these components from the prior analyses, and in line with the arguments reviewed earlier, equation (3) states that the effects of party dominance and turnout on coups also depend on the ethnic composition of the population.[12] If there is no

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[12] Multicollinearity is not a problem with this model. In fact, correlations among the independent variables are low ($N = 29$):

	M	*C*	*D*	*P*
Social Mobilization (*M*)	1.00			
Ethnic Dominance (*C*)	–.249	1.00		
Party Dominance (*D*)	–.419	.152	1.00	
Turnout (*P*)	–.074	–.038	.253	1.00

such dependence, none of the additional parameters (i.e., β_6, β_7, and β_8) will be effectively different from zero.

Given the earlier arguments and the provisional results, let me outline more specifically the effects that we might expect from these additional parameters. First, the presence of a dominant ethnic group appears to have destabilizing consequences, while single party dominance appears to have the opposite effect. The stabilizing effects of party dominance may be particularly pronounced where there is a potentially dominant ethnic group, since the integrative role of strong parties may help transcend the disintegrative effects of ethnic dominance. This perspective implies $\beta_6 < 0$. In contrast, a positive estimate for this parameter would imply just the opposite, namely, that the destabilizing effects of ethnic dominance are not diminished by strong parties, and that the stabilizing effects of strong parties operate only when there is a series of smaller ethnic groups.

Second, the joint effects of ethnic dominance and turnout may operate in one of two ways. One possibility ($\beta_7 < 0$) is that the destabilizing effect of single ethnic-group dominance can be overcome by higher political participation. An alternative outcome ($\beta_7 > 0$) implies that the destabilizing effects of ethnic dominance may be increased by electoral turnout. Either of these outcomes would point to the significance of situations involving high turnout *coupled with* ethnic dominance.

Finally, the last parameter (β_8) in equation (3) allows for a second-order interaction between ethnic dominance, high turnout, and party strength. Given that the first two of these variables are binary, this specification means that I am allowing for a final adjustment to the slope for party strength for those cases ($N = 8$) where there is a dominant ethnic group *and* high turnout. Contrast this with the first-order interactions (β_5 and β_6), which allow for adjustments to the slope for party strength when *either* ethnic dominance *or* high turnout is present. A positive estimate for β_8 would imply weaker party-strength effects when *both* of the other conditions are met, while a negative estimate would imply just the opposite.

Table 3 reports the estimates for two versions of equation (3), one with β_8 constrained to a value of zero, and the other with no such constraint. A very striking feature of both sets of estimates is the fit of the model. To be sure, I have specified a model that in statistical terms involves several parameters. At a theoretical level, however, only four explanatory variables are included, and the model accounts for four-fifths of the variance in elite instability in black African countries in the years between 1960 and 1975. Even when one adjusts for the loss of degrees of freedom resulting from the number of parameters, the fit of the model is impressive.

These estimates also reveal that there is no support for the more complex version of equation (3) that specifies a second-order interaction: the estimate of β_8 in the second column of Table 3 is smaller than its standard error and the corrected coefficient of determination in the same column is slightly smaller than it was in the first column. This means that party dominance has no special effects in countries with *both* high turnout *and* a dominant ethnic group. We can therefore confine our attention to the estimates of equation (3) in the first column of Table 3. In Figure 1, estimated Coup scores from this model are plotted against actual *Coup* scores.[13] Only the Sudan has a residual (12.959) that is larger than the standard deviation of the coup d'ėtat index (12.510), and even this difference is slight. In general, then, this scatterplot suggests no obvious biases in the model, but rather reinforces the earlier impression that the model fits well.

Briefly, the estimates in the first column of Table 3 suggest that each of the four variables has a pronounced effect on elite instability. All of the eight parameter estimates are precise (including $\hat{\alpha}$), in that three of them have *t*-ratios larger than 3.0, while the smallest (that for β_5)

Note incidentally that the binary-variable versions of *C* and *P* are retained in equation (3), since they provided optimal estimates. Thus, this equation constitutes a rather detailed analysis of covariance.

[13] To check on the assumption of homoskedastic disturbances for this model, I followed two procedures. First, as suggested by Anscombe and Tukey (1963), I plotted the fitted values ($\widehat{Coups}$) against the residuals ($Coups - \widehat{Coups}$). Second, as suggested by Johnston (1972, p. 219), I computed Spearman coefficients of rank correlation between the *absolute* values of the residuals and each of the independent variables in equation (3). Neither of these constitutes a formal "test" for homoskedasticity, but because they are nonparametric, both procedures have the advantage that they do not require (unavailable) information on the precise form of the process generating any heteroskedastic disturbances. Neither of these procedures provides evidence of heteroskedasticity in the estimates in the first column of Table 3.

is close to 2.0 (1.68).[14] In addition, these estimates indicate that none of the effects from the analyses reported in Tables 1 and 2 is spurious. The additive positive effects of mobilization and ethnic dominance on elite instability persist, while the separate and joint effects of participation and party dominance described in Table 3 are of form similar to (if slightly weaker than) those reported in Table 2.

At the same time, the figures in Table 3 point to two important elaborations of the results in the last two sections. These modifications concern the joint, nonadditive effects of ethnic dominance and party dominance and turnout, respectively, shown in the negative estimate for β_6 and in the positive estimate for β_7. I shall deal with these in turn.

First, the estimate for β_6 indicates that the stabilizing effect of party dominance is much more pronounced in countries where a dominant ethnic group exists. Where there is no dominant ethnic group, the slope for party dominance ($\hat{\beta}_3$) is –.202, whereas the slope for party strength is more than *doubled* in countries where the largest ethnic group constitutes at least 44 percent of the population ($\hat{\beta}_3 + \hat{\beta}_6 =$ –.519). This suggests that the integrative role of a strong political party is particularly important when an ethnic group is of a size that makes it potentially dominant.

Second, the estimate for β_7 is positive, which means that electoral turnout does not inhibit coups in countries with a dominant ethnic group as effectively as it does elsewhere. Where a more fractionalized ethnic population exists, the effect of turnout ($\hat{\beta}_4$) is –23.047. However, where a dominant group exists, this effect of turnout is *halved* ($\hat{\beta}_4 + \hat{\beta}_7 =$ –11.667). An important implication of these results is that party strength is a more potent stabilizing force than mass political participation in the form of turnout in countries with a dominant ethnic group. In contrast, the effects of these

Table 3. Regressions of Coups d'état, 1960 to 1975, on Mobilization, Pluralism, Party Strength, and Turnout (N = 29)[a]

M	Social Mobilization	.227 (.073)	.205 (.079)
C	Size of Largest Ethnic Group	27.666 (7.142)	23.578 (9.041)
D	Percent Vote to Winning Party	–.202 (.091)	–.245 (.108)
P	Electoral Turnout	–23.047 (7.838)	–29.114 (11.315)
D.P	Winning Party Vote * Turnout	.186 (.111)	.281 (.169)
C.D	Ethnic Group Size * Winning Party Vote	–.317 (.104)	–.252 (.136)
C.P	Ethnic Group Size * Turnout	11.380 (4.529)	23.570 (16.866)
C.D.P	Ethnic Group Size * Winning Party Vote * Turnout		–.179 (.239)
Constant		19.129 (7.326)	22.487 (8.649)
R^2		.843	.847
$\bar{R}^2$		.791	.786

[a] Ethiopia is excluded from this analysis. Main table entries are the parameter estimates, and numbers below them in parentheses are their standard errors.

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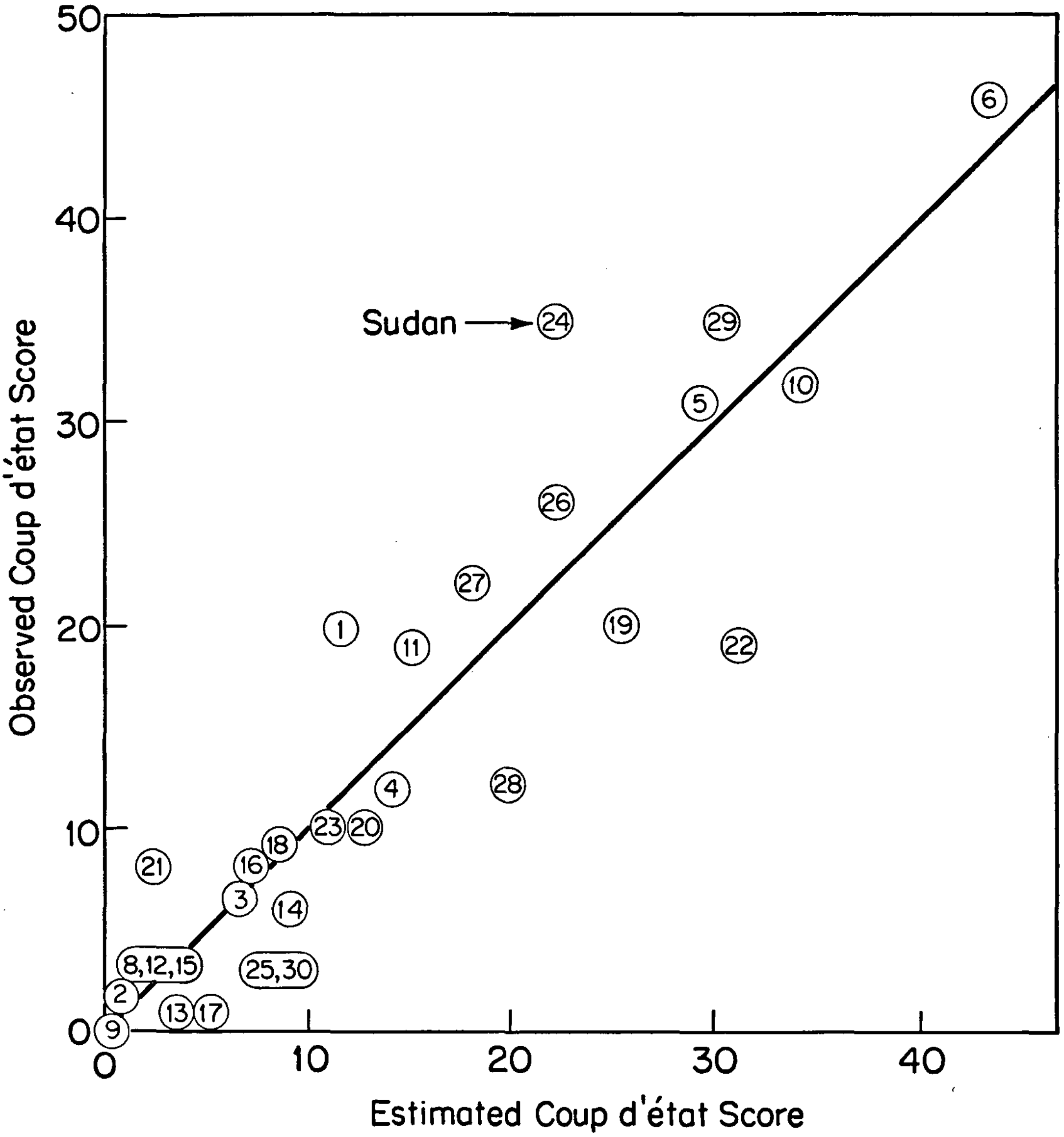
[14] The fact that $\hat{\beta}_5$ has a *t*-ratio slightly less than 2.0 does not warrant its exclusion from the model, since reestimating equation (3) subject to the constraint that $\beta_5 = \beta_8 = 0$ produces estimates for β_3 and β_4 that are approximately half the size of those reported in the first column of Table 3. In addition, this revised estimate of β_3 has a *t*-ratio of 1.55. This means that β_5 should be retained in the model so that the nonadditive effects of party dominance and electoral turnout are not suppressed.

two political variables are similar in ethnically plural societies.[15]

Summary and Implications

This paper has centered on coups d'état—those illegal efforts by insurgent elites (typical-



[a] Estimated coup d'état scores are from the estimates in the first column of Table 3. Country numbers are provided in the Appendix.

Figure 1. Plot of Observed Coup d'état Scores Against Estimated Coup d'état Scores ($N = 29$)[a]

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[15] This judgment depends on the size of the coefficients weighted by the range of the relevant variable. Party dominance (D) ranges from 16 to 100, while electoral participation (P) is either 0 or 1. Thus, in societies with no dominant ethnic group, the effect of D $(-.202*100 = -20.2)$ is similar to the effect of P (-23.047).

ly the military) to bypass normal or constitutional channels of executive succession that are accompanied by actual or threatened resorts to physical violence. In particular, we focused on an index covering 16 years that is a weighted sum of successful, unsuccessful, and plotted coups, respectively.

For students of African affairs, one clear inference to be drawn from this study is that *instability of this kind is not random with respect to political and social structure.* Assuming that I have excluded no important independent variables and (even) that my measures are perfectly reliable, this study shows that the idiosyncratic (i.e., random) factors to which Zolberg (1968, p. 71), Decalo (1976, p. 22) and others have alluded cannot account for more than one-fifth of the variance in coups d'état (the presence of unreliability would, of course, further lower the residual variance that we might attribute to idiosyncratic factors).[16]

Instead, the fit of this model that casts coups as a function of structural factors is striking. Far from pointing to a random process, these estimates suggest a rather *deterministic* pattern. It is evident from Figure 1 that the volume of elite instability in the countries of black Africa from 1960 to 1975 can be estimated with a remarkable degree of accuracy, given four straightforward and distinct conditions that are central to the theoretical literature on political instability. Three of these conditions clearly predate the dependent variable, while the fourth (measured c. 1965 to 1966) is a variable that probably underwent little pronounced change in the preceding six years.

Besides these clear implications for African politics, the specific results of this analysis bear on more general discussions of political instability and change. First, social mobilization has a strong, linear destabilizing effect. It is important to remember that this pattern was found among countries that achieved political independence quite recently, which means that these countries do not meet Huntington's initial criterion for institutionalization. The positive effect for mobilization found here is therefore quite consistent with Deutsch's and Huntington's hypothesis concerning the destabilizing effects of social mobilization in countries whose governments lack political capacity.

Second, this analysis suggests that cultural pluralism has important consequences for political instability. However, in contrast to the usual view (e.g., Deutsch, 1961; Rabushka and Shepsle, 1972), it appears that ethnic diversity (that is, cultural pluralism) is a *stabilizing* force. Political instability seems to result when one group's size (at least 44 percent of the population) makes it dominant. This implies support for the Madisonian view that the presence of such dominant groups prevents the formation of countervailing power centers that help inhibit political instability (c.f., Dahl, 1956, Ch. 1; Coleman, 1960, p. 368). Presumably, the formation of broad coalitions in favor of a coup or the elimination of alternative coalitions that might mount a counter coup is inhibited as the ethnic diversity of a society increases. Thus, the present study affirms the centrality of ethnicity to the study of African politics; it suggests that the presence of a large and potentially dominant group has destablizing results. Note, however, that no support was found for the view that variations in ethnic pluralism alter the effect of social mobilization on coups.

Third, the results show that party dominance is stabilizing (while multipartyism has the opposite effect). This suggests that one-party dominance is probably an integrative force. Such an interpretation is strengthened by the analysis of the *combined* effects of party dominance and ethnic dominance. The size of the already pronounced stabilizing effect of party dominance is more than doubled as we move from plural societies to those in which there exists a dominant ethnic group (the coefficient for party dominance changes from –.202 to –.519). This pattern indicates that multipartyism is particularly destabilizing when coupled with the presence of a dominant ethnic group.

Fourth, the results indicate that increased electoral turnout *decreases* the probability of coups. This is consistent with Deutsch's emphasis on the stabilizing effects of political participation, where the latter, by increasing the number of politically relevant persons, diminishes the chances of successful covert illegal political activity instigated by small numbers of people. However, my argument is slightly different from his, since a different pattern holds for the effects of *social* mobilization (which, it will be remembered, is uncorrelated with turnout in these African countries). I think these results suggest that regardless of the degree of *social* mobilization, *political* mobilization in the form of higher levels of mass electoral participation may reflect a higher degree of acceptance of conventional, nonviolent processes of elite succession. If this acceptance is in fact widespread at the time of independence, it may mean that subsequently the population and the politically relevant strata are less likely to respond favorably to violent and extraconstitutional attempts to seize power. Such a pattern would be consistent with the observed stabilizing effect of higher turnout at independence. Note that such considerations imply that Huntington's emphasis on the need "to restrict or to control political mobilization" (1968, p. 425) may be misplaced. Normal forms of *mass* political participation seem generally to be as salient as party strength when it comes to creating a stable political order.

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[16] Of course, I am not claiming that these measures are perfectly reliable: as I have already noted, potential sources of unreliability are extensively discussed by Morrison, et al. (1972). The fit of the model does suggest, however, that unreliability is not a major problem (simply because its presence would be reflected in attenuated correlations). In conjunction with the parameter estimates, the fit of the model can also be viewed as evidence favoring the validity of the measures I have used.

The principal qualification to this statement is that the inhibiting effects of mass turnout on coups are weaker (by a factor of one-half) where a dominant ethnic group exists than they are in more plural societies. This means that in the former situation, party strength is more important than voter turnout. I hasten to add that even then, the effect of turnout on instability remains pronounced and negative.

Finally, the estimates provide no support for the argument that party strength has a unique impact on elite instability in countries with high turnout *coupled with* a dominant ethnic group. At the same time, the estimates *do* indicate that turnout and party strength have a nonadditive effect. More specifically, the stabilizing effect of party strength is quite pronounced when turnout is low, but it disappears where turnout is high. This implies that the stabilizing roles of party strength and voter turnout may be somewhat interchangeable.

In short, the analysis suggests that both social mobilization and the presence of a potentially dominant ethnic group have destabilizing consequences, at least in the context of the new nations of black Africa. The first of these variables is one that changes relatively slowly, while the second is even less responsive to conventional political action. However, the results indicate that the destabilizing results of these two "social" variables, especially ethnic dominance, are substantially reduced by two "political" factors (mass participation and party strength). My emphasis on the role of strong political parties is hardly original. More novel is the clear implication of this analysis that the stabilizing impact of *mass* political participation may generally be as important as is the consolidation of political parties (an activity that is primarily in the hands of political elites). This suggests that besides its uncomfortable normative implications, the common argument that restricted participation brings stability may be more myth than fact.

Appendix: List of Countries

1. Burundi
2. Cameroon
3. Central African Republic
4. Chad
5. Congo-Brazzaville
6. Dahomey (Benin)
7. Ethiopia
8. Gabon
9. Gambia
10. Ghana
11. Guinea
12. Ivory Coast
13. Kenya
14. Liberia
15. Malawi
16. Mali
17. Mauritania
18. Niger
19. Nigeria
20. Rwanda
21. Senegal
22. Sierra Leone
23. Somalia
24. Sudan
25. Tanzania
26. Togo
27. Uganda
28. Upper Volta
29. Zaire
30. Zambia

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Dissolving International Politics: Reflections on the Nation-State

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The diminishing salience of territorial issues, the restraints imposed by the nuclear balance, the shift away from the primacy of military-strategic elements of power to the primacy of economic elements, the day-to-day realities of economic interdependence, and changes in the nature of the nation-state have produced a new international order which resembles in important ways the domestic political systems prevalent in the industrialized noncommunist part of the world. This leads to the "domestication" of international politics. At the same time competitive nationalism, the vitality of the nation-state, differing perceptions of the proper role of government in the economy, and other considerations allow not much more than a tenuous coordination of foreign policies even among similar nation-states, making it unlikely that the European Economic Community and the Atlantic alliance will proceed beyond existing structures toward tighter integration.

Two distinctive forces act on the modern nation-state, and through it, on contemporary international politics. On the one hand, the welfare demands of its citizens have pushed the modern nation-state toward a peak of power and activity unprecedented in its 300-year history. Whatever a country's institutional arrangements, stages of economic growth, or ideological preferences may be, remedies for the economic and social problems of the individual are sought in public policy and collective action. Politics has become the primary arena for the redistribution of income, status and other public satisfactions. Politics everywhere extends into wider areas, touching upon aspects of public and private life that in the past have escaped governmental scrutiny as well as governmental solicitude. The modern state is pervasive in its activities; assertive of its prerogatives; and powerful in what it can give, take or withhold.

On the other hand, the power of the state, although obtrusive and dominant in its domestic context, appears compromised in rather novel ways in its international context–in part because of the restraints imposed by the nuclear balance of terror, and in part because the domestic power of the state can be sustained only through international economic cooperation and political accommodation. In order to meet its responsibilities for mass social and economic welfare, the modern state is compelled to interact with other states in ways which, although not lacking in conflict and competition, demand cooperation, acceptance of the logic of interdependence, and a willingness to condone restraints on state behavior and sovereign prerogatives. Internal state power is sustained by external cooperation.

These two forces acting on the nation-state carry with them conflicting as well as complementary implications about the nature of the contemporary international system; and they lead to questions about the balance between independent and interdependent state activities, between security concerns and welfare concerns, between conflict and cooperation, and between domestic and international politics.

Nationalism and the Contemporary International System

The assertive character of the nation-state is reflected in a "new nationalism," a phenomenon that has a deep impact on global politics. The forces of nationalism, aside from the inhibitions created by the nuclear balance, have proved to be the major restraint placed upon the conduct of the superpowers in the period after World War II. In their attempt to create a world order congenial to their ideological preferences or to their national interests, both the United States and the Soviet Union have had to contend with the stubborn appeal of nationalism, inside as well as outside of their respective alliances. The fissures appearing in both the North Atlantic Treaty Organization and the Warsaw Treaty Organization during the last two decades are in large part attributable to the resistance of secondary alliance members to making their policies conform to the guidelines set forth by their alliance superpower. In many instances, this resistance is based not so much on ideological grounds–nationalism as a counter-ideology to international ideologies–but on pragmatic considerations which suggest that differences among national socioeconomic, cultural and political circumstances warrant different definitions of the public good and require divergent paths toward its realization. The "new nationalism," although not lacking in emotional overtones, is supported by rational calculations on how to further the national interest within global and regional configurations of power in which the superpowers still exert an overwhelming measure of influence. Nor are these calculations directed solely toward the superpowers. Secondary powers engage in competitive nationalism among themselves, especially in regional ventures such as the European Economic Community where conflicting interests rub against one another abrasively precisely because they are packed together closely.

National divergencies continue to resist attempts to streamline and coordinate policies within alliances. Both the United States and the Soviet Union have responded to "deviationists" within their alliances with a good deal of exasperation; and both have sought to contain the centrifugal forces within their spheres of influence as much as possible, although in practical terms they have dealt differently with challenges to their hegemony. Moreover, the United States at times has viewed the problems experienced by the Soviet Union in Eastern Europe with a measure of guarded sympathy. The much-noted remarks by Helmut Sonnenfeldt in December 1975, in which he characterized the relationship between the Soviet Union and Eastern Europe as "unnatural" and "inorganic," and therefore potentially destabilizing and a threat to world peace, seem to indicate that the United States perceived the forces of nationalism to be detrimental to American interests even when they weaken the opponent's sphere of influence. To what extent Moscow's hostile reaction to Eurocommunism may stem from a similar calculation is a matter of speculation.

Both superpowers also have found it onerous to cope with the forces of nationalism in the Third World. Many of the new states in the Third World are going through a process of self-definition, and their governing elites have used the rhetoric and symbols of nationalism to create or enhance a national identity that is ill-defined if not entirely lacking. It is not surprising that those in power seek to enhance national consciousness and the corresponding legitimation of their rule by stressing the notions of separateness, of a distinct identity, that are inherent in the ideology of nationalism. This explains in part the lack of success of international ideologies in many Third World states. Aside from the dubious applicability of ideological prescriptions in solving immediate problems of governance, adherence to international ideologies would diminish the measure of independence and separateness that these states have achieved only in the recent past. Although the governments of some of these countries describe themselves as Marxist or socialist, there is little indication that this means adherence to an internationalist Marxist ideology. Any internationalist or cosmopolitan value system is bound to have limited appeal in states that are engaged in the process of self-definition, perceived primarily in national terms.

The Soviet experience in the Middle East, and in several African countries has demonstrated that the Soviet Union is at least as inept as the United States in attempting to steer political and economic development in the Third World on the basis of ideological prescriptions. In their dealings with the Third World, both the Soviet Union and the United States have persistently and seriously underestimated the forces of nationalism. American opposition throughout the late 1950s and 1960s to so-called "neutralism" of the Third World was based in large part on a misreading of the psychological and political factors that motivated the elites in Third World countries. Part of the reason for the American débacle in Vietnam (aside from the futile attempt to apply measures of containment that had proved successful in Europe to areas in which they were totally inappropriate) was an underestimation of the driving forces and appeal of nationalism in Southeast Asia. Ho Chi Minh was as much a nationalist as he was a communist, and the war in Vietnam was a war of reunification as much as a war of ideology. As John Lewis Gaddis has put it, "compared to such entrenched phenomena as nationalism, racism, greed, or sheer human intractability, communism is today a relatively insignificant determinant of events on the international scene, unless of course we choose to import significance to it by giving it more attention than it deserves" (1977, p. 885).

The Domestication of International Politics

Nationalism, then, is alive and well. Far from being secondary or obsolete, the nation-state, nationalism, and the idea of the national interest are central elements in contemporary world politics. The international system has remained an interstate system in many of its essential features. At the same time equally powerful forces are at work which have modified the role of the nation-state, broadening its capacity to shape events in some respects, narrowing it in others. These forces are in part the result of the changing nature of the nation-state itself and in part the result of new ways in which nation-states interact. They are developments which go to the roots of the perennial preoccupations of the state: welfare and security.

The meaning of national welfare and the approaches toward its achievement are profoundly affected by the major change in the nation-state that I mentioned at the beginning: its growing responsiveness to the revolution of rising expectations or, as Daniel Bell calls it in a somewhat sharper term, the "revolution of rising entitlement" (1977). Modern governments have become increasingly sensitive to demands for a wide variety of welfare services and have taken on responsibility for mass social and economic welfare. The improvement through state intervention of the material (and perhaps even psychological) well-being of its citizens has become one of the central functions of state activity. The satisfaction of rising claims by citizens has become a major source of the state's legitimation and of a government's continuance in office.

This has led to an intensive flow of interactions, of social demand-and-supply communications between the state and society, through which politics and the bureaucracy rather than the market have become the major agents for social change and the redistribution of wealth and power. But the demands which are generated and processed through these "vertical" interactions on the domestic level can be satisfied only by extensive commercial, monetary and technological interactions on the international level. Three types of processes are available for this purpose. There are the "horizontal" interactions among the units of world politics, on the government-to-government level, which take place in bilateral as well as multilateral settings. This is the stuff of traditional international politics. There are "lateral" interactions, also called "transnational," which are the society-to-society dealings across national boundaries among subnational groups and organizations, such as multinational corporations, international banks, export-import firms, professional organizations, coordinating and consultative arrangements among national political parties, labor unions, guerrilla organizations, and so forth. (Although the participants in this type of transaction are "private" or "semi-public," their juridical and political status differs from country to country—a point to which I shall return later.) Another type of interaction is "integrative," involving supranational processes (such as those of the European Economic Community) which are institutionalized and have to some extent diminished national prerogatives.[1]

In sum, there has developed on the global level an interconnected and intensified flow of national-vertical, international-horizontal, transnational-lateral and supranational-integrative processes—a complex of relationships, usually described as interdependence, in which demands are articulated and processed through formal as well as informal channels, governmental as well as non-governmental organizations, national as well as international and supranational institutions. These processes of interaction are interdependent—that is to say they are a system—and they perform a variety of functions, most prominently those of welfare and security. They are the structures through which governments perform a variety of functions; they are the ways in which state and society seek to arrange their domestic and foreign environment. But even in a highly interdependent global system national governments have ample discretion as to what structures, what types of interactions, they wish to employ for performing certain functions. To put it more precisely: the choice of one structure over another is determined as much by internal ideological, institutional, and political orthodoxies as it is by external necessities. Most trading relationships in the industrialized noncommunist parts of the world are handled in transnational processes, with national governments deciding how "private" the enterprise system is allowed to be; in communist countries international trade is a state activity. Security issues everywhere are traditionally processed on the international, government-to-government level; as are such important economic issues as formal currency devaluations and tariff policies. Supranational processes, as exemplified in the European Economic Community, tend to be limited to essentially economic interactions.

In what follows, I shall try to demonstrate that the bulk of today's global political processes are of a kind that are typical of and approximate domestic political processes, leading to the "domestication" of international politics; and that, contrary to the expectations of functionalists and other theorists, it is not a new type of international politics which is "dissolving" the traditional nation-state but a new nation-state which is "dissolving" traditional international politics.

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[1] There is an additional, "mixed" type of interaction in which one party is a governmental agency and the other party is a private firm. These "diagonal" interactions are typical of many East-West and North-South economic dealings.

Five aspects of the contemporary global political system have a bearing on my argument. First, interdependence requires a permissive context; it is possible only in a type of international system that allows it. "Liberalization" of trade and money flows, minimal interference with transnational investment activities, absence of protectionism, and other "liberal" economic preferences—as well as the political purposes and ideological justifications connected with them—are prerequisites for a highly interdependent political and economic system. Although it is technology that has shrunk the world, politics has kept it that way. International economic systems, as much as military-strategic and political systems, reflect the influence and interests of their predominant members. The international economic and monetary arrangements of the postwar period were essentially the creation of the United States, which emerged from the war as the undisputed economic and monetary superpower. Although conceived initially as a worldwide arrangement of liberalized trade and monetary relations—a global "open door" for the United States—following the onset of the cold war the arrangement began to revolve around the trilateral relationships among the United States, Western Europe, and Japan, with the communist economic-monetary system becoming a regional sub-system. This trilateral combine was subjected to increasing stress during the 1960s and underwent major changes in the early 1970s, with the result that the United States has had to share its predominant position with Western Europe and Japan. A new international monetary and trading system is developing, but its shape is still ambiguous, especially since the impact of the North-South conflict and of OPEC's monetary resources is as yet uncertain.

Second, although domestic demands can be satisfied only by intense participation in international or transnational activities—providing governments with powerful incentives to cooperate with one another—nationalism nonetheless can thrive in a context of interdependence just as interdependence can survive competing nationalisms. Richard Rosecrance and Arthur Stein suggest (1973, pp. 21, 5) that

> under the stimulus of economic nationalism . . . nations may also occasionally act against the multilateral framework. . . . Nationalism might have been expected to reduce interdependence. It might be argued that, if nations seek only to achieve their own goals without reference to the rest of the system, the linkage between units must decline. If nationalistic goals depend on supportive actions by other members of the international community, however, nationalism cannot be achieved in isolation. Not only does interdependence not decline in such circumstances, aggressive nationalism may lead to higher negative interdependence. The greater nationalism of the twentieth century therefore need not entail a reduction of interdependence.

A third point is that in an interdependent system, whether global or regional, domestic political conflicts over the redistribution of wealth and power may extend into the transnational, supranational, or international context. This affects the disposition of issues. Schattschneider says (1964, p. 2): "The outcome of all conflict is determined by the *scope* of its contagion. The number of people involved in any conflict determines what happens; every change in the number of participants, every increase or reduction in the number of participants affects the result." Whether the constituency for conflict resolution is enlarged in an interdependent system depends on the extent to which national governments permit transnational and supranational processes to take place. If these processes are curtailed by governmental restrictions, the scope of conflict remains localized, with the government acting as the gatekeeper between internal and external demand flows.

The same process can work in reverse. Political conflict may be projected not only from the domestic onto the international scene but international conflicts over redistribution of income may be projected onto domestic political scenes. National governments have always been at the fulcrum where foreign and domestic policies meet, where conflicting pressures have to be weighed and adjusted, where the perennial scarcity of resources requires hard choices and rank-ordering of priorities. Governments have to manage two interlocking processes of redistribution of power, influence, and wealth. In most contemporary societies, the government engages in a continuing process of redistributing domestic power and wealth. It does so whether it is an "activist" government or whether it is content to let "market forces" make the redistribution. A redistribution takes place in either case. By not acting, the government also acts. At the same time a national government is confronted with a continuous redistributive process in the international system, a constantly shifting configuration of power. In states where the national government allows or encourages a wide range of transnational "private" interactions–where the government partially forswears the role of gatekeeper between internal and external environment–international redistributive processes reach into national redistributive processes more easily because they are not checked by governmental interposition.

As a result, and this is my fourth point, a new convergence of international and domestic political processes is under way in the industrialized noncommunist parts of the world, with consequences that are most likely irreversible but are neither fully understood nor perhaps fully acceptable. In some major respects, governments find it increasingly difficult, or meaningless, to distinguish between foreign policy and domestic policy. Nowhere is this more clearly visible and institutionalized than in the operations of regional international organizations that are endowed with some measure of supranational authority, however limited. It is difficult to distinguish between domestic and foreign policy in an institution whose policies have consequences that cannot be assessed in terms of either purely external or purely internal consequences. But the fusion of domestic and foreign policy takes place even in the absence of supranational processes; it reflects a process in which the traditional boundaries separating the nation-state from the environing international system are becoming increasingly obscured and permeable.[2]

The fifth point, which is of central importance, is that security issues have diminished in salience relative to economic issues. Although security can become a question of national survival in the nuclear age–and in that sense is unsurpassed in importance–a noticeable shift of emphasis has taken place in world politics, away from the primacy of military-strategic elements of power toward the primacy of economic elements. For one, the likelihood of invasions and direct military aggression has receded, especially in areas which are basically unattractive objects of physical aggression and territorial occupation. Except in parts of the non-industrialized world and in the Middle East, territorial revisions are not a pressing issue in modern international politics. A number of years ago, John Herz argued that for centuries the major attribute of the nation-state was its "territoriality": its identification with an area that was surrounded by a "wall of defensibility" and hence relatively impermeable to outside penetration. This territoriality was bound to vanish, so Herz argued, largely because of developments in the means of destruction, such as nuclear weapons, which made even the most powerful nation-state subject to being permeated (Herz, 1957). Although Herz later modified his views on the future of the nation-state (Herz, 1968), his argument on the changed meaning and importance of territoriality was clearly valid.

The diminishing salience of territorial issues, the restraints imposed by the nuclear balance, and the day-to-day realities of economic interdependence have changed the meaning of power in global politics. Access rather than acquisition, presence rather than rule, penetration rather than possession have become the important issues. Often one gains the impression that negotiations over such technical questions as arms control, trade agreements, technology transfers, and monetary reform are not only attempts at problem-solving but also re-examinations of the meaning and sources of power in the last third of this century. Many military-strategic and economic issues are at bottom political issues couched in technical terms.

This has led a number of analysts to argue that "low politics" has replaced "high politics" as the stuff of international politics. There is a good deal of truth in this; and the distinction is a useful one although it should be sharpened. For one, the dichotomy between "high" and "low" politics, between the pursuit of security and power (the dramatic-political-intangible) and the pursuit of welfare and affluence (the economic-incremental-tangible) can be overdrawn. Karl Kaiser was correct when he suggested a number of years ago that what political actors view as either high or low politics depends on specific circumstances, changes over time, and in any case may be different from country to country (Kaiser, 1967). Also, there is a difference between high and low politics that has not been sufficiently stressed and that is pertinent to my argument: power, security and defense commodities are indivisible, and hence less subject to the redistributive aspects of political processes, whereas welfare issues are divisible and at the very core of redistribution politics. Goals such as power and security are public goods and subject to the calculus of relative gain. Goals pertaining to welfare, economics and "profit" are private goods and can be assessed with respect to absolute gain. To put it another way: high politics pertains to indivisible collective goods whereas low politics pertains to divisible private goods.[3]

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[2] In addition to its practical consequences, the fusion of domestic and foreign policy raises some fundamental theoretical issues for the analysis of international and domestic politics. (See Hanrieder, 1968, 1971).

In combination, the five features of global politics that I have enumerated suggest that international politics is subject to a process of "domestication." In particular, the more international political processes concentrate on activities that are distributive the more they resemble traditional domestic political processes. This development is fed from two sources, as I have tried to demonstrate. On the one hand, the diminishing salience of security issues relative to economic issues narrows the area of "high" nondistributive politics and enlarges the area of "low" distributive politics. At the same time, distributive processes have increased in frequency as well as in intensity—nationally as well as internationally and transnationally. It isn't so much that welfare issues have emerged as high politics, as some authors would suggest, but rather that distributive political processes have gained in relative importance, and that the mounting demands generated within a society cannot be satisfied without recourse to international and transnational processes. As governments rely on external transactions to meet domestic demands, distributive politics on the international and national levels have become intermingled, leading to a fusion of domestic and foreign policy in the area of distributive politics. In order for this to happen, both international and domestic circumstances must be appropriate. The international system must be sufficiently stable, predictable, and permissive for extensive transnational processes to take place; and national political systems must feature political, institutional, and ideological attitudes that accept these processes.

It is precisely the domestication of international politics which sustains (and demonstrates) the vitality of the nation-state. By extending domestic political processes and their corresponding attitudes into the international environment, the nation-state has eroded traditional aspects of international politics. Many analysts in the postwar period perceived the major change in international political, economic, and strategic processes to come from a gradual weakening of the nation-state. Transnational and international processes were expected to modify the nation-state. Modern international politics was to dissolve the nation-state. What has happened, however, is that the modern nation-state has "dissolved" a certain type of international politics as the importance of nondistributive processes diminished relative to distributive processes.[4]

This is not to deny the continuing importance of security issues and the extensive residual of traditional international politics that is still visible in global processes. The contemporary international system is a mix of traditional and novel processes, and its essence lies in the dialectic relationship between the old and the new. This dialectic is reflected in what has happened to the idea of the "national interest." The concept of the national interest is, practically by definition, an idea based on nondistributive, indivisible values, enjoyed by society as a whole: security, prestige, territoriality, political advantages sought in manipulating the balance of power, and so forth. In short: the idea of the national interest is synonymous with "high" politics. As international politics becomes more "domesticated," the policy areas covered by the concept of the national interest become more narrow and ambiguous. Distributive values, unlike nondistributive values, are not shared equally by all segments of society. Since the idea of the nation-state and the national interest have been used in an almost symbiotic sense, at least in traditional parlance, it seems ironic that while the salience of the nation-state has been enhanced, for the reasons enumerated, the analytical usefulness of the term "national interest" has been seriously diminished.

Domestication and the Trilateral Paradigm

More specifically, what does "domestication" of international politics mean? What domestic model of political and economic processes, of what state or what type of state, is being approximated by international processes? What type of nation-state "dissolves" international politics?

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[3] The indivisible nature of the demands dealt with by high politics gives them a zero-sum quality. Territorial disputes are highly volatile because of the exclusiveness of territorial possession; A's security may mean B's insecurity; A's prestige may be achieved at the expense of B's. Lowi (1964) makes a distinction among distributive, regulatory, and redistributive domestic political processes, which is extended to foreign policy issues by Ripley and Franklin (1976).

[4] The title for this essay suggested itself to me after reading an excellent review article by R. Harrison Wagner (1974), entitled "Dissolving the State: Three Recent Perspectives on International Relations." It seemed to me that precisely the opposite was taking place: namely, that new activities of the nation-state were "dissolving" a certain type of international politics.

The Third World. One type of domestic order that is not approximated by the general international system is that of a typical Third World country—if one may apply the term "typical" to such a diverse part of the world. One of the most fundamental distinctions between developed and underdeveloped countries is the way in which demands for the redistribution of public and private goods are generated, articulated, aggregated and conveyed to the government for satisfaction. Most LDCs are deficient in private or semi-public institutions that can process societal demands vertically or seek benefits laterally, that can operate inside as well as outside of their political systems. Underdeveloped countries tend to be "state-dominant" systems rather than "society-dominant" systems because their political and bureaucratic institutions serve primarily to administer governmental demands downwards rather than respond to societal demands made upwards. Vertical interaction is fragmentary and erratic in both directions. Lateral-transnational interactions are equally tenuous because there are few private organizations or distinct class interests that could interact with their counterparts in other societies. One result is that the redistributive demands made by LDCs on the international scene generally have to be made by their governments. Demands are articulated by the state rather than by society, pressed forward horizontally rather than laterally.

Moreover, in those LDCs that have emerged only recently from colonial domination, many of which are characterized by artificial borders and strong centrifugal forces, it is only natural that their governments should emphasize, in their quest for self-definition, the cohesive rather than the fragile features of their system. In this situation, a state's legitimacy is usually more effectively asserted through its foreign than through its domestic policies. The reality of a new nation, its uniqueness and integrity, are more convincingly expressed in its external relations than in divisive domestic political processes. Foreign policy is put in the service of nation-building.

This trend toward "statism" is reinforced by the establishment of an administrative "new class" in many LDCs, whose political activities are not genuinely revolutionary, in the sense of leading toward a fundamentally restructured social order or creating one de novo. In the absence of a broad middle class, these elites have become "conservative" themselves, turning themselves into an administrative bourgeoisie (Chaliand, 1977). All this supports the view that the political and economic dealings of the LDCs tend to be state-managed and consequently employ international rather than transnational structures for the articulation of demands. "Statism," the absence of effective private or semi-public organizations, the definition of nationhood through external activities, the rhetoric of nationalism, and (in some cases) the quest for boundary revisions lend a certain old-fashioned quality to the foreign policies of many LDCs. They are deeply shaped by the precept of "high" politics.

The Soviet and Chinese Model. Neither does the prevailing "domestication" of international politics approximate the Soviet and Chinese models, which are also characterized by the notions of "high" politics. There are in fact significant parallels between the Third World model and the Soviet and Chinese models, above all perhaps the "state-dominant" mode of dealing with internal and external demand flows. Both the Soviet and Chinese governments preempt commercial and monetary activities; and the integrative features of COMECON are not supranational (in the sense the term is applied to the European Common Market) but rather international since they effect redistributions of income on the government-to-government level. This explains in part why the Soviet Union has found it so difficult to expand its regional economic subsystem on the global level. In addition to suffering from basic economic and monetary weaknesses, which propel the Soviet Union to seek extensive trading arrangements with the West on a barter basis, government-to-government transactions tend to be inadequate because of sluggish bureaucratic processes and because the Soviet Union cannot across the board provide Third World countries with extensive capital investments, stable raw materials prices, and an effective voice in rearranging the global monetary system. What the Third World needs the most, the Soviet Union can provide the least. The frustrations engendered by these difficulties—and there are many more which I cannot discuss in this limited space—may also explain the apparent Soviet inclination to seek expansion of its global influence through traditional power politics, tinged with gun boat diplomacy. This may be a way of compensating for Soviet disabilities in the area of commercial and monetary instruments. One writer, much worried about growing Soviet military strength and its apparent expansionist intent, writes that "Russian imperial strategy has already emerged fully formed in the classic mold. . ." (Luttwak, 1978). This may well be the case, but it raises the question whether the "classic" mold is the best way of extending influence in a world system characterized by economic interdependence and the relative decline of usable military power relative to economic power. In my view, most features of the contemporary international system place the Soviet Union at a large disadvantage relative to the economic power of the trilateral industrialized world.

In any case, and that is really the point that matters in the context of my argument, it can be demonstrated that the prevailing domestication of international politics is not the result of Soviet or Chinese political and economic activities, which have not been very effective in shaping basic global processes.

The Industrialized Trilateral Model. It is apparent that the domestication of international politics follows a paradigm that stems from the internal processes of highly developed industrialized societies, primarily those of the United States, Canada, Japan, and Western and Northern Europe. These processes are characterized by frequent and intense interactions—vertical, horizontal and lateral—and by the prevalence of demands which might be summarized by the term "middle class." These countries are not only similar with respect to the types of demands that percolate within and among them, but they also have available to them a similar set of policy instruments with which to direct, contain, expand or otherwise affect both vertical-domestic as well as horizontal-international and lateral-transnational processes: security policy, trade and monetary policy, fiscal policy, income policy, wages policy, labor policy, taxation policy. There is a wide array of instruments with which modern governments and their bureaucracies can manipulate national as well as international demand flows: similar functions are performed with similar policy instruments.

Not the same, however, are the domestic "structures" (formal-institutional as well as informal-habitual) through which the demands are channeled, and which reflect the values that determine what instruments of public policy are considered legitimate. In the first place, such notions as "middle class" demands are in themselves different in different societies. The meaning of "middle-class values" is not comprehensible apart from other values and traditions that could be summarized as "political culture" and "economic-sociological culture." (It is surprising, in fact, that the concept of "economic culture" has not gained the same prominence as that accorded to "political culture.") In addition, the division of labor between state and society, the mix between public, semipublic and private structures, the relations between interest groups and bureaucracies, the redistributive use of taxation, sensitivity to inflation and unemployment, attitudes on economic growth, preoccupation with national security—to mention just a few examples—are significantly different in different industrialized societies. The domestic political and economic structures in industrialized countries, and the explicit and implicit code that defines the appropriate role of government in the economy and society, are quite divergent. These divergencies have a great impact on the type of policies that are pursued, how they are implemented, how readily they can be coordinated internationally, and to what extent they permit or resist "linkages" in alliance politics. In other words, we must now draw further distinctions within the general paradigm of "industrialized societies" and consider how these distinctions affect economic interdependence and intra-alliance cooperation. This can be demonstrated most poignantly in the case of European economic integration and the transatlantic relationships between the United States and Western Europe.

Coordination of Politics and the Nation-State

From the beginning of European integration and of the coordinating features of the Atlantic alliance, two contradictory processes (at times of unequal intensity) have been visible: a process of divergence and a process of integration. These contradictory trends have been analyzed in a long series of academic publications, and a review of this literature, as well as of the public debate about the issues themselves, need not detain us here. One might suggest, however, that the processes of "coordination" of policies among nation-states in the European and Atlantic communities occupy a middle ground between the tendencies toward divergence and the tendencies toward integration. A spectrum of policies and attitudes emerges that goes from divergence to "parallelism" to coordination to integration, ranging from minimal cooperation to maximal institutional collaboration.

All members of the European and Atlantic communities have at different times, for different reasons, and on different issues, pursued all four categories of policies. There are a number of well-known examples of policy divergencies as well as of integrative processes: de Gaulle's decision to remove France from the unified command structure of NATO (as well as other Gaullist foreign policy projects) is an example of policy divergence, whereas the establishment of the European Community is an example of an integrative type of policy.

Located as they are at the two extremes of the divergence-parallelism-coordination-integration spectrum, such examples tend to be the most dramatic. It seems to me, however, that the more pressing issues in transatlantic and intra-European processes are located in the middle ground of the spectrum, in the areas of parallelism and coordination.

There is no question that in many important respects the political systems of Western Europe have become more and more alike. But parallelism has not impelled them toward more integrative structures but, at best, toward more coordination of national policies. This is so not only because of internal domestic obstacles but also because each member of the European Community has a distinctly different relationship with the United States.

But at the same time governments must employ horizontal and encourage lateral transactions in order to satisfy the vertical demands pressed upon them by their electorates, which can be ignored only at the risk of being removed from office. It is primarily for this reason that the coordination of policies has become a central issue in intra-European as well as transatlantic relationships. Parallel developments, the similarity of domestic problems and of public demands, require some measure of international and transnational cooperation. But since the intensification of integration is unacceptable to many members of the European Community for a variety of reasons, coordination appears to be the only alternative. Policy coordination has become a substitute for integration.

Should the Community be enlarged in the next few years, the prospects for deeper integration become even more remote. But there is a question right now whether there exist compelling economic and monetary reasons for giving community institutions more power, or whether it is sufficient to solidify and streamline them. As Leon Lindberg and Stuart Scheingold have pointed out several years ago (1970), important industrial and commercial interests in the Community are interested primarily in sustaining the present level of integration, seeing their interests adequately served by the status quo and shying away from the uncertainties and readjustments which attend changes in the scope and intensity of supranational arrangements. Solidification and rationalization rather than intensification is the key phrase here—and the trend is as pronounced now as it was years ago. The "expansive logic of sector integration," as Ernst Haas called it, seems to have turned into the "status quo logic of sector integration," a logic which welcomes the existing measure of integration but turns to coordination for solving new problems rather than go beyond it.

The same applies to monetary policy. A decade ago, Lawrence Krause noted a phenomenon that is even more pronounced now than it was then, and that has contributed (along with other important reasons) to the postponing of a monetary union within the Community: "Governments do not need to be told . . . that excessive inflation in an open economy quickly leads to difficulties for themselves and their trading partners. They can see for themselves the rapidly deteriorating balance of payments, and pressures immediately arise for corrective actions. A 'hidden hand' toward policy coordination is directed by the market mechanism and it has proven to be very effective with the EEC" (Krause, 1967, p. 24). Leaving the joint-floated currency snake (an "integrative" device) toward the coordinating device of hammering out monetary assistance measures among the Community members is a typical example from the more recent past. While the infirmities of the dollar have provided added incentives for fashioning a coordinated European monetary policy, it is unlikely that this will lead to monetary integration. The creation of a common European currency would be practically the equivalent of political union since it requires that the central prerogatives remaining with national governments—in economic, monetary, social, fiscal and foreign policy—would be given over to supranational institutions. Again, coordination rather than integration appears to be more acceptable.

The possibilities for coordination are uneven in the area of indivisible goods, such as security issues and "high politics" foreign policy issues. The Western stance at the European Security Conference was fairly well coordinated, but this was so in large part because West Germany's *Ostpolitik* and the resulting treaty arrangements had already resolved issues that were vital to the Soviet Union and Eastern Europe. A coordinated European foreign policy is as remote now as it has ever been; and it is difficult to imagine events that would push the Community in that direction, especially in a decade in which governments are less inclined to pursue grandiose schemes for global and regional power rearrangements than in the 1960s.

Coordination on security issues is a particularly instructive example, for here one must distinguish between security as an end—which may be an "indivisible" product for an alliance as well as for a nation-state—and the means with which that end is achieved (say, weapons procurement) which can be a highly divisible commodity. Whether security is indivisible in the Western alliance, and in its regional European NATO component, is an uncertainty that has plagued NATO for almost two decades. With the institutionalization of strategic nuclear "parity" and "equivalence" in the SALT accords, Washington's European NATO partners (and especially the Federal Republic) can hardly feel reassured about the willingness of the United States to meet a conventional attack with a nuclear response. It is still the central paradox of NATO strategy that in dealing with the Soviet Union the United States must implicitly recognize strategic parity whereas a convincing extension of American nuclear protection to Europe implies American superiority. Were it not for the fact that direct military aggression is highly unlikely, the fissures within NATO would be wider and deeper than they are. As it is, the issue has been swept under the rug, and when it tends to reappear—which happens whenever the Europeans see or imagine reasons to question American resolve—the rug is simply moved to cover it up again. With respect to the "indivisibility" of regional defense, the changes of the French military posture, as exemplified in the ideas of French chief of staff, General Guy Méry, appear somewhat more encouraging since they reflect the recognition that the narrow space of Western Europe requires a common defense. In the transatlantic context, conflicts of interests have been contained through the coordination of contingency planning, as exemplified in NATO's Eurogroup. In the European regional context, the coordination accomplished within the Western European Union contrasts with the early 1950s when integrative rather than coordinating arrangements were envisaged, such as the plans for the aborted European Defense Community.

Highly divisible aspects of security policy—weapons procurement, weapons standardization, cost-sharing arrangements, and so forth—make coordination much more difficult. The same is true in energy policy and raw materials policy, because high politics tends to mingle with low politics along the lines I have redefined these terms earlier.

Aside from conflicting interests, the obstacles to policy coordination stem from differences among national styles of problem solving and decision making. Even if the problems and their apparent solutions were the same in different countries (which they are not), there would be different ways of approaching them. In each country there are entrenched administrative practices that are unique and that resist *international* or *transnational* coordination. While the bureaucratic instinct may be universal and timeless, it cannot be stripped totally of its local historical and institutional context. Equally important, in each country powerful juridical, political and ideological traditions have developed which circumscribe the proper role of government in the economy and society—to use a simple phrase for a highly complex reality. These traditions, and their structural manifestations, are different in different countries. The differences are especially pertinent in policy processes that are distributive rather than non-distributive; they appear in their starkest form in welfare concerns rather than in security concerns. Although governments everywhere are pressured to direct the solution of economic and social problems, their impulses and capacities to act are energized and inhibited in different ways.

A number of years ago, Cyril Black (1966, p. 49) pointed out that "there is much evidence that Japan and France, the United States and the Soviet Union, or Mexico and Poland are becoming more alike functionally. . . . What they do is becoming more similar, but the way they do it remains different in significant respects." The same point has been made persuasively in a number of more recent analyses.[5] Peter Katzenstein (1976, pp. 19, 44), for example, argues that the differing relationships between state and society (as well as other related factors) in the United States and France have a profound impact on the success of coordinating economic and monetary policies:

> The similarity in the policy networks linking state and society will determine the degree of similarity in government responses to problems of the international economy. The joint impact of international effects and domestic structures thus condition government policy. . . . Consistency and content of policies . . . are the two primary dimensions which affect the coordination of policies between states. . . . In the French-American case government policies diverged along both dimensions, thus raising the greatest problems for the coordination of policies. The corporatist policy networks in the

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[5] A whole new "trend" is visible in the number of analyses that focus on internal, "subsystemic" processes and thus stress the differences rather than the similarities among nation-states and their foreign policies. See, among many other examples, Frank and Hirono (1974); Frohlich and Oppenheimer (1972); Hansen (1973); Hayward and Watson (1975); Heidenheimer, Heclo and Teich Adams (1976); Hoffmann (1974); Krasner (1972); Lindblom (1977); Morse (1976); H. and M. Schmiegelow (1975); H. and W. Wallace and Webb (1977); Wallace (1976).

Federal Republic, to take another example, generate a foreign economic policy which is reminiscent of French policy in its great consistency but resembles American policy in its economic content. Policy coordination between West Germany and France as well as West Germany and the United States has, therefore, been more successful than between France and the United States.

Nationalism and Interdependence

The processes I have described reflect a dialectic of independence and interdependence. In advancing their interests, governments and subgovernmental groups, society as well as the state, have brought about interdependence. Interdependence is sustained because these interest calculations do not allow the disintegration of interdependence toward a more fragmented and contentious international system but neither propel it toward more integration and supranationality. Interdependence, and the coordination required for its operation, is a halfway house between disintegration and integration of political and economic processes. Interdependence is the prototypical phenomenon of an international system that derives its dynamics from the pursuit of the national interest as well as of interests that are narrower and larger than the national interest.

It must be stressed again that the term "national interest" in this context is ambiguous and can be misleading. Distributive goods, which are the bulk of interdependence processes, are not shared equally by all segments of society as is the case with nondistributive goods, such as security. If we cannot even properly apply the term "national interest," with its rationalistic overtones, it would appear to be even more misleading to use the term "nationalism," with its emotive, irrational and atavistic implications—implications that correspond much more to the nation-state concept that we see as being eroded by various permeative processes. The concept of nationalism is analytically outdated, focusing as it does on an irrelevant view of territoriality and carrying with it the assumption that the nation, incorporated by its people, represents an organic whole. In short, the terminology of nationalism is inappropriate precisely because it rests on the notion of indivisible values and the corresponding idea of "high politics," at a time when most day-to-day political and economic processes are of the divisible kind.

I am aware of the paradox of having stated earlier that "nationalism is alive and well," and suggesting now the inapplicability of the term itself. But there is a difference between a concept and a sentiment. The idea of the "nation" as a communal organization still elicits feelings of commitment and hence enriches public life. This "psychological" nationalism should perhaps be viewed as a quest for continuity when traditional values are changing and the possibilities for identification with a larger purpose are diminishing. The secularization of both theological and political ideologies brings with it an agnostic pragmatism which provides little more than a utilitarian view of public life. The theme of the "end of ideology," tattered as it is, still explains a good deal.

Interdependence also narrows the opportunities for national self-identification. The contours of a national identity become nebulous precisely because the interests and values advanced in processes of interdependence cannot be unequivocally defined and experienced in national terms. In part they continue to be national, but they are at the same time larger and smaller—global as well as municipal, cosmopolitan as well as provincial. The nation-state, the social and cultural environment within which most citizens continue to define their spiritual and material well-being, has become deficient in providing that well-being—at the very least in its material sense, but most likely in a spiritual sense as well. Governments, in seeking to meet the demands pressed upon them by their electorates, are compelled to turn to external sources in order to meet these demands. But their reluctance to opt either for divergence or for integration places them in an area of ambiguity where coordination appears as the reasonable as well as the necessary course of action. And yet the obstacles to coordination arise from the differences among industrialized societies and their governmental structures, although the needs that coordination is intended to meet are common and widely shared.

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The Stochastic Process of Alliance Formation Behavior*

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This article attempts to examine alliance formation behavior as a stochastic process amenable to systems interpretation. We test two hypotheses linking the systems characteristics of bipolar distribution/tightly knit bloc structure to serial dependence and stability of alliance formation behavior and multipolar distribution/loosely knit bloc structure to randomness and process instability.

For testing these hypotheses, we have introduced a modeling strategy whereby identification of serial dependence and parameter estimation are accomplished by means of stochastic difference equations and time series autocorrelation and partial autocorrelation functions, and systems interpretation is facilitated by employing its continuous analogue in differential equation form.

We analyze data on three sets of observations corresponding to three distinct periods of international relations: 1815–1914, 1919–1939, 1945–1965. The results do confirm the proposed hypotheses revealing randomness and instability of alliance formation behavior in the multipolar/loosely knit periods of 1815–1914 and 1919–1939 and serial dependence and stability in the bipolar/tightly knit 1945–1965 period. Finally, we consider the implications of these results for the earlier findings, the balance of power model, and the probability of war.

The centrality of alliance behavior to the observation and interpretation of international politics was first established by such classical writers as Kautilya, Thucydides and Machiavelli. More contemporary scholars have continued to reaffirm this privileged position (e.g., Modelski, 1963; Singer and Small, 1966a, 1966b, 1968; Russett, 1968, 1971; Burgess and Moore, 1973; Holsti, Hopmann and Sullivan, 1973). Despite the impressive array and diversity of the literature, a number of questions about alliance formation behavior remain unresolved, just as many of the empirical findings must be considered tentative. Moreover, only a few studies have treated alliance behavior as a stochastic process. Understandably, our current knowledge of the stochastic dynamics of alliance formation behavior remains thin at best.

To date, several studies (Job, 1973; Siverson and Duncan, 1973; McGowan and Rood, 1975) have directly attacked this question concerning the stochastic dynamics of alliance formation behavior. The most frequently addressed problem is whether alliance formation behavior is a random process or if successive states of alliance formation behavior are serially dependent over time. The results are somewhat mixed. Job, for instance, fitted a Poisson distribution to observed data and found no systematic trends in the 1815–1965, 1815–1870, and 1945–1965 periods, but considered it possible for them to have existed in the 1871–1914 and the 1919–1939 periods. Siverson and Duncan, in contrast, used two additional tests, the negative binomial and the Yule-Greenwood, and observed "contagious-heterogeneous" effects in the 1815–1965 period, though they discounted the possibility of similar effects in other periods. Meanwhile, McGowan and Rood found a close fit between the random Poisson process and the alliance formation series for the 1815–1914 period. Thus the only clear-cut consensus applies to the finding of serial dependence in the entire 1815–1965 period.[1]

In addition, two of the three studies also seem to lack a coherent theoretical perspective that gives substance to the empirical findings. The notable exception is the McGowan and Rood study in which the authors searched for evidence of randomness within a balance of

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[1] This lack of consensus is due in part to the interdependence/heterogeneity assumptions inherent in the comparison of "Poissonian" models (see Li and Thompson, 1975).

power construct for nineteenth-century Europe. Their results, according to the authors, did confirm the hypothesis that, under the balance of power system, alliance formation behavior was more or less randomly distributed. Even here, though, the analysis is not problem-free. Putting aside the arguments for and against its meaningfulness, the balance of power construct is customarily restricted to pre-1914 international politics. The limitation of the construct becomes immediately apparent when it is confronted with a more inflexible alliance structure, as in the post-World War II period, in which constant realignments for the purpose of avoiding hegemony by any single actor were noticeably absent. Thus, in order to consider a wider range of systems, we must establish a broader theoretical construct than the "balance of power theory."

We undertake this analysis in the hope of contributing to this ongoing research by addressing the above-mentioned problems of restrictive methodology and theoretical substance. Specifically, we will address two questions that will further explicate the dynamics of alliance formation behavior: (1) Does the alliance formation process appear to be random over time with previous behavior exerting little or no influence on future behavior, or is it serially dependent with the number of alliances at one time exerting a contagion effect on the number of alliances at a subsequent time? And, if so, what is the "order" of that serial dependence? (2) Viewing alliance formation behavior as a dynamic process, is it possible to determine whether it is stable or unstable—by which we mean, is there resistance to alliance proliferation or a tendency toward proliferation in the process? To assist in answering these questions, we will introduce a general construct that not only serves to organize our inquiry and permits us to deduce the dynamic properties of alliance formation behavior but also resolves the difficulty of inadequate explanations of alliance formation behavior.

The general construct we will introduce rests on two basic concepts, "capability distribution" and the "tightness of bloc structures." For the sake of simplicity we will focus on the extreme combinations of bipolar/tightly knit (B/T) bloc structures on the one hand and multipolar/loosely knit (M/L) bloc structure on the other. We hope that through these structural characteristics, we will be able to infer the nature of alliance formation dynamics insofar as serial dependence, the order of dependence, and the stability of the dynamics are concerned. We will test empirically the information so gained in terms of two hypotheses. For purposes of testing and interpretation, we will entertain a general methodology whereby data analysis is performed with a discrete stochastic process model and systems interpretation facilitated by an equivalent model in continuous form.

Capability Distributions, the Tightness of Bloc Structures and Military Alliance Formation Processes

As structural characteristics, the distribution of capabilities and the tightness of bloc structures have figured significantly in recent analyses of world politics. The primary concern with which they have been associated involves the relationships between the broad conceptions of "polarity" and "polarization" on the one hand and systemic "stability" (e.g., the absence of war) on the other (see Waltz, 1964, 1967; Deutsch and Singer, 1964; Rosecrance, 1966; Haas, 1970; Goldmann, 1972; Nogee, 1975). Without sifting through all of the possible combinations of linkages and the various meanings attached to the terms in the literature, we will consider in this analysis a simple dichotomous construct which pits B/T structure against M/L structure and which links these mutually exclusive systemic attributes to the stochastic process characteristics of serial dependence/stability and independence/instability, respectively. Since there is no commonly accepted interpretation of these concepts, we must first define them.

By *capability distribution* we mean the number of great powers which qualify as possessing no less than a specified proportion of the system's available military capability defining a potential major power pole (a more precise formulation will be given later). If only one great power qualifies, the system's capability distribution is considered *unipolar;* if two, it is *bipolar;* if three or more, it becomes *multipolar.* In short, our use of the term capability distribution refers to the structural characteristics specifying the number of major powers at any one time possessing at least a specified proportion of the system's total military capabilities. In contrast, by the *tightness of the bloc structure* we refer to the centripetal behavioral process by which actors tend to cluster around major powers to form mutually exclusive blocs or coalitions. The range of bloc tightness runs the gamut of extremes from the tightly knit case where the major actors form two blocs or camps with predominantly conflictual inter-bloc relations and primarily cooperative intra-bloc relations, to the loosely knit case where competitive and cooperative relations are diffused among the major actors, and bloc identifications, if they exist, are short-lived and lacking in total commitment.

These definitions are hardly novel, for they represent the central structural variables usually encountered in the polarity/polarization literature. However, several recent critiques (Goldman, 1972; Modelski, 1974; Hart, 1974; Bueno de Mesquita, 1975; Caporaso, 1976), have noted that, though they are distinct systemic attributes, capability distribution and the tightness of bloc structure may covary over a range of values. For instance, a system with a bipolar (multipolar) capability distribution may possess either a tightly or a loosely knit bloc structure. But for our purpose we will use only the extreme combinations of B/T structure and M/L structure. Though these are simple and ideal type combinations,[2] they offer the advantage of eliminating consideration of combinations in between, which would have raised insurmountable problems of theory and measurement.

Hypotheses. With regard to the linkages we posit between serial dependence or independence on the one hand and degrees of capability distribution and tightness of bloc structure on the other, we begin with the observation that according to the relevant literature, systems with a B/T structure are characterized by two leading actors and a single dominant source of cleavage. This cleavage exerts pressures on the other actors in the system, particularly the more important ones, to gravitate toward one of the two leading poles. As the system begins to resemble two hostile camps, the significance of relative improvements in position and resource standing among actors increases and the very structure of the system accentuates this significance by stimulating reciprocal suspicion between the two camps. One side's improvements elicit the other side's counteraction, for it is feared that even relatively slight advantages may lead to a fundamental change in system structure—at the expense of the disadvantaged camp.

In direct contrast, systems which have M/L structures are associated with three or more major actors and with multiple sources of cross-cutting cleavages which work to reduce the intensity of any single conflict. In comparison with the B/T case, minor structural changes in M/L systems are of less obvious importance. Since adversary relationships in an M/L system are less intense and multiple, the immediate consequences of relative improvements by one major actor are not clear to the other major actors. Nor is there as much structural incentive as in a B/T system for other actors to exaggerate the extent of direct threat perceived. Instead, the threat posed by any one actor's position and resource gains tends to be diffused throughout the system.

In brief, changes in military capabilities are more likely to be closely monitored and perhaps magnified in a B/T system than in a M/L system. Countervailing reaction to any changes in capabilities is therefore more likely in the former type of system than in the latter. Since military alliances represent one important way in which military capabilities may be augmented or at least given the appearance of being strengthened, the formation of military alliances among some actors is more likely to provoke countervailing actions by others in a B/T systems than in a M/L systems. This should be particularly so as the alliance structure crystallizes into two major opposing blocs. If this is true, we can expect alliance formations to be much more interdependent and non-random over time in B/T systems than in their M/L counterparts. In the aggregate, such an alliance formation series is expected to appear serially dependent over time. Our first research question can then be translated into the following testable hypothesis:

H1: The formation of military alliances is more likely to be a serially dependent process in an international system characterized by B/T structure than in one characterized by M/L structure.

Moreover, the constructs of capability distribution and tightness of bloc structure also lend themselves to the interpretation of other aspects of systems dynamics—in particular, stability, instability, and homogeneous instability. In systems theory, stability implies the presence of a permanent equilibrium position within a process such that, if perturbed and displaced, the process will always return to equilibrium. Instability, in contrast, implies the absence of a permanent equilibrium point. In these circumstances, a displaced process either continuously veers away from the point of initial impact or eventually evolves and settles about a new equilibrium position. If the divergence from equilibrium continues unabated, the corresponding process is said to be unstable; if it drifts from one equilibrium position to another, it is considered to be homogeneously unstable.

Returning to the structural properties, the existence of only two major blocs in a B/T system severely reduces the autonomy of actors clustered around the polar leaders and limits the freedom of actors to leave their bloc either to join the opposing camp or to assume a neutral position. By definition, as long as polarization tendencies remain intense, once actors have joined one of the blocs, they will tend to remain in their respective clusters. This tendency is facilitated by systemic forces such as a common ideological bond, systemic tension, high level of perceived threat, and the dominance of polar leaders over the subordinate actors. In sum, this suggests that movements among alliance members will be restricted to limited alternatives in a B/T system and that the maintenance of these limited alternatives will enhance the stability of the equilibrium position. Conceivably, the greater the tightness about the bipolar centers, the steadier is their structural equilibrium about which the alliance formation process evolves.

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[2] More attention is given to these problems in Rapkin, Thompson, and Christopherson (1977).

In contrast, the presence of multiple poles, multiple issues and the absence of alignment rigidity in M/L systems permit relatively unrestricted movement by alliance members. The political mobility possible in such a system would make it unfeasible to maintain a permanent equilibrium position. Still, the degree of instability may vary with the level of polarization in a multipolar system. Where alliance actors are only weakly drawn toward the various polar centers, any significant perturbation may displace the system from equilibrium and create a chain reaction that ultimately results in total collapse of the alliance structure. This is a highly unstable situation. But where there is some attraction to polar centers, perturbation will quickly wear off, leading to the establishment of a new equilibrium position. In this case, we have an unstable process. Accordingly, this suggests that the alliance structure of a M/L system is likely to be less stable than the alliance structure of a B/T system. However, since we cannot be very precise about the interaction pattern between capability distributions and the tightness of bloc structures, this last observation about mixed situations must remain speculative.

Nonetheless, whether military alliance formation processes exhibit one of these dynamics must be verified empirically, not speculated about. Thus the relationship between capability distributions, bloc structure tightness, and process stability noted above may be stated in the following testable hypothesis:

H2: The process of military alliance formation is more likely to be stable in a system characterized by a B/T structure than in one characterized by a M/L structure.

In the following sections we will (1) operationalize the variables of capability distribution, bloc structure tightness, and military alliance formation behavior; (2) establish a mathematical model of serial dependence as an alternative to the poisson models previously employed; and (3) set down the mathematical definitions of stability and instability. Operationalization and the mathematical framework will then facilitate the testing and analysis of the two previously stated hypotheses.

Data, Operationalization, and Test Criteria

Military Alliance Data. The Correlates of War (COW) project's list of alliances, involving formal military commitments for the period 1815–1965, serves as our central data source (Singer and Small, 1966a; Small and Singer, 1969). Only those instances in which new alliances were negotiated by treaty or old alliances renegotiated were counted as alliance formation events.

The COW alliance data is restricted to peacetime agreements. We have excluded pacts negotiated by governments either participating in war or within three months prior to participation unless the alliances managed to survive the war unaltered. It is not clear how many alliances are excluded by this qualification, although, according to one source (Holsti, Hopmann, and Sullivan, 1973, pp. 234–36), some 18 wartime alliances were created between 1815 and 1939: 1854 (6); 1859 (1); 1866 (1), 1914 (1); 1915 (3); 1916 (2); 1917 (11); 1939 (3). Since we are interested in global alliance formation processes and in their relationship to the tightness of bloc structures, we would naturally prefer to include wartime agreements; but a standardized listing for the 1815–1965 period is not currently available. Fortunately, any bias which might be introduced by ignoring wartime alliances will be mitigated by a decision on our part to subdivide the 1815–1965 series into three subseries: pre-World War I (1815–1914), interwar (1919–1939), and post-World War II (1945–1965).[3]

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[3] Inasmuch as the interwar period and the post-World War II period contain series with little more than 21 observations, one can expect the precision of the subsequent estimates to be compromised. But since our interest is not forecasting but modeling, we

While our decision to regard the 1815–1914 period as a single series distinguishes this period from the further subdivisions found in a number of other studies (Rosecrance, 1963; Singer and Small, 1968; Singer, Bremer, Stuckey, 1972), our practice is generally consistent with the preference shown among studies of alliance formation behavior cited earlier. Moreover, our preliminary analyses for some alternative subseries (e.g., 1815–1869, 1870–1914) have also confirmed the McGowan and Rood observation (1975) that the entire 1815–1914 period exhibits consistently random alliance formation patterns and may be treated as a single subseries. Any further subdivision will be superfluous.

Three different indices of alliance behavior will be examined: new alliances, great power new alliances, and existing alliances. *New alliances* refer to the frequency of alliances created per year by all states in the system. *Great power new alliances* encompass those alliances which involve agreements between two or more of those states conventionally recognized as great powers. The *existing alliance* category includes the new alliances and previously created alliances still in effect. This third category is examined solely on the chance that the cumulative number of alliances in the system may reveal a different aspect of alliance formation behavior than the annual number of new alliances. Similarly, the great power category is examined in case global patterns should mask the presumably more crucial behavior of the major actors. Since the international system has expanded considerably in size, both the new and existing alliance data have been detrended for system size by regressing the original alliance data on the annual number of states in the system. We have then taken residuals from the regression equation as the detrended data. The number of great powers has also fluctuated over the entire 1815–1965 period (from four to eight) but the range of fluctuation has not been of such magnitude within each of the temporal sub-periods to warrant detrending the great power series.

Capability Distributions. Adapting Modelski's (1974, p. 2) operational definition of polarity, a system with a bipolar distribution is one in which two great powers each possesses at least 25 percent of the available military capabilities and together hold more than 50 percent of that power. Contrastingly, a multipolar system is one in which three or more great powers each claims at least 5 percent of the available military capabilities, but no two surpass 25 percent, and all together possess at least 50 percent of such power.[4]

Table 1 lists a number of "relative capability" scores for the nine great powers operating during the 1815–1965 period. It should be noted that the COW concept of relative capability is multidimensional and thus encompasses more than military capabilities alone.[5] This,

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can tolerate the extra bias so long as our estimates are statistically significant within reasonable confidence intervals and provide clear indications of the direction of relation. In either case, our estimates proved to be satisfactory.

[4] While we are specifying three qualifying actors as the minimum situation for multipolar distributions, in the periods we are examining the actual number of qualifying actors ranges from five to eight.

[5] The relative capability score taps six indices: total population, urban population, energy consumption, iron and steel production, total military expenditures, and size of armed forces (see Singer, Bremer, Stuckey, 1972, pp. 25–26).

Table 1. Relative Capability Scores for the Great Powers, 1816–1965

	Proportion of Relative Capabilities in the Great Power Subsystem								
Great Powers	**1816**	**1840**	**1865**	**1890**	**1913**	**1938**	**1946**	**1955**	**1965**
England	42.0	35.5	35.3	29.3	14.0	10.0	15.2	8.7	6.8
Russia (USSR)	25.5	19.7	19.0	17.8	16.7	25.3	26.3	26.0	29.2
France	16.5	22.8	18.7	15.0	10.3	8.0	6.2	5.8	5.0
Austria-Hungary	12.0	13.5	9.3	8.2	6.3	–	–	–	–
Prussia (Germany)	4.5	8.7	9.7	22.0	18.0	18.7	–	–	–
Italy	–	–	7.7	7.7	5.0	4.7	–	–	–
Japan	–	–	–	–	4.5	10.0	–	–	–
United States	–	–	–	–	24.8	23.5	52.7	40.5	36.7
China	–	–	–	–	–	–	–	19.2	22.2

Data Source: Raymond F. Hopkins and Richard W. Mansbach (1973). *Structure and Process in International Politics.* New York: Harper and Row, p. 111.

plus the fact that the percentages are based on great power subsystem aggregations rather than world totals, forces us to use the scores with some caution. Nevertheless, the capability distributions depicted for 1840, 1865, 1890, 1913, and 1938 appear to categorize the international system as multipolar for those years. The 1816 distribution suggests instead a bipolar distribution with England and Russia constituting the two leading powers. While we cannot demonstrate this at present, this was probably a short-lived situation in the post-Napoleonic War era. If we may further generalize the results for specific years depicted in Table 1 to include adjacent points in the 1814–1914 and 1919–1939 periods, then these two eras of the international system can be construed as predominantly multipolar in terms of capability distribution. In contrast, the capability distributions displayed for 1946, 1955, and 1965 appear to qualify the 1945–1965 period as essentially bipolar even though the 1946 scores suggest something more like a unipolar distribution, again presumably short-lived.

Tightness of Bloc Structures. With respect to the tightness of bloc structures we currently lack a satisfactory and sufficient longitudinal index capable of capturing the fluctuations in the behavioral tendencies of actors to cluster in mutually exclusive blocs or in coalitions around major power poles. Consequently, we are forced to rely on a combination of historical impressions and partial empirical evidence to arrive at an operational definition. Some fairly sophisticated work relating to polarization processes has been done with alliance data (Bremer, 1972; Wallace, 1973; Buena de Mesquita, 1975) but these efforts are limited for our purposes because they do not discriminate between minor and major power blocs. In the conceptual perspective we are advancing, only great powers are of interest as potential poles around which bloc formation and disintegration occur. That is to say, in a system with two great powers, for instance, there are only two potential poles or possible bloc leaders; in a five-power system, there are five potential poles or possible bloc leaders. The small states, because of their lack of sufficient military resources necessary to exert significant influence on global politics and global political structure,[6] will be considered in this analysis only insofar as their memberships in the great power blocs are concerned. Because of the lack of better indicators and given the great power bias in world politics, one approximate way of looking for polarization patterns is to identify interactions between great powers. If the oligarchs of world politics cluster themselves into two distinct groups, we have reason to believe that the bloc structure is tightly knit. To the extent that the major powers do not cluster themselves into two mutually antagonistic groups within the great power subsystem, we would have reason to believe that the bloc structure is not tightly knit, at least in the context we are addressing.

Given this perspective, we can obtain one partial and decidedly crude indicator of the tightness of bloc structure by examining the network of military defense pacts between great powers.[7] Table 2 lists the average number of great power defense pact clusters aggregated by roughly five-year intervals. Each great power which has no formal defense connections with any other great powers is counted as a cluster, as is each discrete group of connected great powers. Reading down the columns in Table 2, we may reach several tentative conclusions about bloc structure. First, apparent tendencies toward the tightness of bloc structure fluctuate throughout the 1815–1965 period. Second, there appear to be a number of sequential increases and decreases in tendencies toward tightness in the 1815–1914 period: increasing circa 1815–1940; decreasing circa 1841–1875; increasing circa 1876–1895 and circa 1900–1914. Third, the 1919–1939 period resembles a top-sided inverted U-shaped pattern increasing in the 1930s while the 1945–1965 period displays the opposite U-shaped pattern with a lessening of tightness in the 1960s.[8] Fourth, focusing on the three aggregate periods, we find that only in the third period (1945–1965) is bloc structure tightly knit (2.3 clusters on the average) with more loosely knit bloc structures characterizing the 1815–1914 and 1919–1939 periods (3.9 and 5.6 clusters on the average respectively). Thus, basing our judgment on this crude evidence (average defense pact clusters), we may conclude that the 1815–1914 and 1919–1939 periods were characterized by relatively loosely knit bloc structures, whereas the 1945–1965 period was characterized by a tightly knit bloc structure.

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[6] This statement may be in the process of becoming historically dated in the sense that the global system seems to be moving away from its traditional bias in favor of military capabilities toward an increasing emphasis on economic capabilities and the possession of raw materials. Traditionally, economic power was converted into military power (or vice versa) and the global power structure was thus kept much more simple than it currently is in the process of becoming. Future research will need to investigate the political consequences of economic capabilities no longer meaning the same thing as the military capabilities which we address in this article.

[7] Defense pacts are alliances committing members to "intervene militarily on the side of any treaty partner that is attacked militarily" (Singer and Small, 1966, p. 5). As such they provide a fair indicator of the extent to which great powers choose sides in world politics. Naturally, we would prefer to supplement this indicator with measures of other interaction dimensions but the relevant data do not yet exist for the entire 1815–1965 period.

In short, we have delineated in the above discussions two parallel patterns of structural attributes linking combinations of capability distributions to degrees of tightness of bloc structure for different subseries of alliance formation process. The patterns which have emerged suggest that the 1815–1914 and 1919–1939 periods were characterized by M/L structures while the 1945–1965 period was characterized by a B/T structure.

Table 2. Average Number of Great Power Defense Pact Clusters, 1815–1965

	Average Number of Great Power Defense Pact Clusters	Sub-Period Averages
1815–1820	1.0	
1821–1825	1.6	
1826–1830	4.0	
1831–1835	3.6	
1836–1840	2.8	
1841–1845	3.0	
1846–1850	4.0	
1851–1855	4.0	
1856–1860	4.2	
1861–1865	4.6	
1866–1870	5.8	
1871–1875	6.0	
1876–1880	5.6	
1881–1885	4.2	
1886–1890	4.0	
1891–1895	3.0	
1896–1900	4.6	
1901–1905	4.2	
1906–1910	4.0	
1911–1914	4.0	3.9
1919–1925	5.7	
1926–1930	7.0	
1931–1935	4.8	
1936–1939	4.8	5.6
1945–1950	2.3	
1951–1955	2.0	
1956–1960	2.0	
1961–1965	2.8	2.3

Test Criteria. In order to confirm hypotheses 1 and 2 some test criteria are required that would call for evidence of serial dependence and system stability for the 1945–1965 period, where alliance formations were subject to operate in a B/T system, but would expect no such evidence for the 1815–1914 and 1919–1939 periods, in which alliance formations were subject to operate in an M/L system. Briefly stated, our corollary test criteria are:

T_1: *evidence of serial dependence and stability in the 1945–1965 alliance formation subseries;*

T_2: *no evidence of serial dependence and instability in the 1815–1914 and the 1919–1939 alliance formation subseries.*

A Time-Series Analysis of Alliance Formation Behavior

From the preceding discussion, it is apparent that two separate hypotheses are being tested, here phrased as questions: (1) Is there serial dependence in alliance formation behavior and, by implication, what is the order of that stochastic structure? (2) What are the systems implications with regard to stability of the values for the parameters generated as measures of serial dependence? To accomplish the first test we will entertain an *m*th order discrete autoregressive model and for the second test we will examine the range of parameter values in a continuous function.

In this connection we will introduce a new technique which permits us to obtain discrete estimates which we then transform into continuous form in order to facilitate a systems interpretation. First, consider the discrete system. A model of serial dependence in alliance formation behavior begins with the assumption that a homogeneous population of alliance formation moves from a state at time $t-1$ to time t and the transitional rate may be defined by a simple parameter ϕ, representing a state of Markov dependence. In general, such dependence may reach back for n states such that a state at time t may be defined as a linear function of states at $t-1$, $t-2$, . . . , $t-n$ as follows

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[8] This 1945–1965 finding is corroborated by current research (Rapkin, Thompson, Christopherson, 1977) which examines 1948–1973 polarization patterns with more comprehensive event interaction data.

$$Y_t = \sum_{i=1}^{n} \phi_i \; Y_{t-i} + a_t \qquad (1)$$

where Y_t stands for frequency of alliance formation and a_t accounts for random variations that are normally distributed with mean zero and variance σ_a^2. The model is known as an *m*th order autoregressive process. To determine whether serial dependence exists, one needs to confirm that one of the ϕ_i parameters is nonzero, but in order to determine the order of serial dependence one must also identify which one or which set of the ϕ_i parameters that are nonzero. Estimation of the ϕ_i parameters would provide the necessary information.

But the foregoing model is limited with regard to a systems interpretation. Beyond the fact of serial dependence between successive states, there is little else we can say about the structure of the alliance formation process. And it is no less difficult to interpret the meaning of discrete parameters for our understanding of stability (resistance to alliance proliferation) in the alliance formation process. For instance, it is well known that for the system in (1) to be stable its characteristic roots λ_i, must lie within the unit interval, $-1 < \lambda_i < 1$.[9] Within this region the roots may be $-.9$, $.01$, $.5$ and $.8$. Does this mean that .8 is more unstable than .5 and .01 more stable than .5, or does .5 imply stability and .01 imply simply no serial dependence? The difficulty lies in the fact that the stability region contains zero as its midpoint, a factor which inhibits clear-cut interpretation.

Fortunately these problems with interpretation may be allayed to a large degree by considering an equivalent system of continuous functions. From classical mechanics, for instance, we have an intuitive interpretation for the first and zero-order parameters of a second-order differential equation, whereby the former, call it α_1, represents the dampening, inertial force, or resistance to change from the equilibrium position (which we take to be zero)[10] and the latter, α_0, as the elastic force proportional to the displacement. Thus as α_1 increases in size the resistance to change increases so the changes that can be induced by external perturbation become marginal and the system becomes more stable. But equally important is the fact that for a continuous function, the demarcation between stability and instability for characteristic roots, u_i, is conveniently set at zero, where the regions below this point are stable (i.e., $u_i \leqslant 0$) and the region on or above (i.e., $u_i \geqslant 0$) is homogeneously unstable or unstable. Thus with zero as the dividing line one might say that $-.9$ is more stable than $-.5$ and the latter more so than .01. Needless to say then, for physical interpretation, it is more expedient to move from discrete to continuous functions, and in a later section we will further demonstrate this point with the estimates in hand.

Now having justified the need to consider an equivalent continuous function for the purpose of expediting a systems interpretation, let us proceed to introduce a stochastic differential equation for the purpose of testing the second hypothesis:

$$D^n Y(t) = \sum_{i=0}^{n-1} \alpha_i D^i \, Y(t) + Z(t) \qquad (2)$$

where $D^n = \dfrac{d^n}{dt^n}$; $Y(t)$ is defined as before; and $Z(t)$ stands for random shocks whose mean and variance are

$$E(Z(t)) = 0$$

and

$$E[Z(t)\, Z(t+k)] = \begin{cases} \sigma_a^2 & k = u \\ 0 & k \neq u \end{cases}$$

and the model is akin to a non-homogeneous linear differential equation with its input being a stochastic series.

This continuous function is linked to the discrete form (1) by the relation (Pandit and Wu, 1976)[11]

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[9] The characteristic roots of an equation are simply the values it must take to satisfy the zero condition (i.e., in a homogeneous equation). For second-order and higher-order models the roots may be real or complex. If the roots, of an *n*th-order model for example, are real then the underlying dynamics of the time series process will be defined by the sum of *n* exponential curves; but if the roots are complex then there will $n/2$ complex conjugate terms defining a series of sinusoidal oscillations.

[10] It can be shown (see Li, 1976a) that the autoregressive process in (1) is transformable into a moving average noise series, i.e.,

$$Y_t = g_1 \sum_{j=0}^{\infty} \lambda_1^j a_{t-j} + \ldots + g_m \sum_{j=0}^{\infty} \lambda_m^j a_{t-j}$$

where $g_1, \ldots, g_m$ are constants and the a_t's are random noises with mean zero and variance σ_a^2.

[11] The correspondence is exact for first-order models. But for second- and higher-order models, the

$$\phi_i = e^{-\alpha_i \Delta} \qquad (3)$$

where $e^{-\alpha_i \Delta}$ is the solution for the continuous function in which the discrete interval Δ is substituted for the lag term $(t-v)$. The foregoing relation is easily transformed into a function of α_i by a logarithmic transformation and by setting $\Delta=1$ which after transposing terms becomes

$$\alpha_i = 1n(-\phi_i).$$

Since we link a positive ϕ to a negative α we have in effect an inverse relationship between the discrete and the continuous parameters which satisfy the following conditions

as ϕ approaches 0 α approaches ∞
as ϕ approaches 1 α approaches 0.

As a nonhomogeneous equation, (2) has a unique solution which may be derived in terms of an orthogonal decomposition. According to Wold's (1938) decomposition theorem, every time series may be decomposed into a weighted sum of orthogonal random functions, which in the context of this analysis means that a complicated serial dependent process in alliance formation behavior may be decomposed into the sum of its component parts of random shocks.

Accordingly, an orthogonal decomposition for (2) may be obtained in the form of

$$Y(t) = \sum_{i=1}^{n} \int_{-\infty}^{t} C_i e^{u_i(t-v)} Z(v)\, dv \qquad (4)$$

where u_i's are characteristic roots of the equation and C's are constant scaling factors as some linear combination of the roots.[12] This solution, as an orthogonal decomposition of the serial dependence process in (2), can be shown to satisfy (2) by direct substitution (see Pandit and Wu, 1976, pp. 6–22 to 6–25).

As for all solutions of stochastic differential equations, the above relation contains information useful for an interpretation of systems dynamics. Since the subject is relatively unfamiliar to political scientists, some preliminary description of the underlying properties seems in order before we examine the dynamics of alliance formation behavior in light of hypothesis 2. There are basically three possible patterns of systems dynamics all of which depend on the values of the characteristic roots.

(1) *Stable dynamics:* if $u_i < 0$, then the exponential terms in (4) tend toward zero as the lag term v approaches infinity. That means the impact of past shocks (decomposed serial dependence) on the present alliance formation state diminishes exponentially as the lag time increases, thus permitting the disturbed process to return to equilibrium (at mean zero). In the context of this analysis, it means that serial dependence in alliance formation behavior diminishes over time.

(2) *Unstable dynamics:* if $u_i = 0$ then the exponential terms in (4) become unity and the resultant function

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correspondence breaks down for complex roots. The connections between the continuous and discrete parameters are no longer unique. This can be explained in terms of the association between second-order roots:

$$u_1 = \frac{1}{\Delta} ln\, (\lambda_1)$$

$$u_2 = \frac{1}{\Delta} ln\, (\lambda_2)$$

where u's and λ's are continuous and discrete roots, respectively. If λ_1 and λ_2 are real, then u_1 and u_2 are also real and uniquely determined. But if λ_1 and λ_2 are complex, then u_1 and u_2 are also complex; because the complex logarithmic functions, *ln* (complex λ_1 and λ_2), are multiple valued, there are infinite values of u_1 and u_2 corresponding to the same values of λ_1 and λ_2.

[12] In order to obtain explicit expression for the constant terms, C_i, we consider a second order model, for illustration, which is

$$(D^2 + \alpha_1 D + \alpha_o)\, Y(t) = Z(t)$$

(assuming unit input) which is equivalent to a homogeneous system $(D^2 + \alpha_1 D + \alpha_o)\, Y(t) = 0$ with initial conditions $Y(0) = 0$ and $Y'(1) = 1$ and solution $Y(t) = C_1 e^{u_1 t} + C_2 e^{u_2 t}$, where $(D^2 + \alpha_1 D + \alpha_o) = (D-u_1)(D-u_2)$ and u_1, u_2 are the roots. Substituting the initial conditions into the solution $Y(t)$ gives us

$$C_1 + C_2 = 0 \qquad \text{for } Y(0) = 0$$

$$C_1 u_1 + C_2 u_2 = 1 \text{ for } Y'(1) = 1$$

Solving these for the constant terms gives us

$$C_1 = \frac{1}{u_1 - u_2} \text{ and } C_2 = \frac{1}{u_1 - u_2}$$

which are linear combinations of the roots. And the explicit solution for the equation is therefore

$$Y(t) = \frac{e^{u_1 t} + e^{u_2 t}}{u_1 - u_2}$$

$$\lim Y(t) = \int_{-\infty}^{t} Z(v)\, dv \qquad (5)$$

is a random walk that drifts at a finite distance from equilibrium. Understandably, when the roots and thus the associated coefficients equal zero, successive states of the alliance formation process should bear no relation to one another. The process associated with this dynamic simply drifts over time without displaying any distinct pattern of behavior.

(3) *Unstable dynamics:* if $u_i > 0$, then the exponential terms in (4) tend toward infinity as v tends toward infinity. When this occurs, the impact of past shocks magnifies, rather than diminishes, over time. Setting off a chain reaction, the related process would explode away from equilibrium. In other words, given an unstable dynamic, serial dependence would magnify over time.

Comparing the estimated values with these stability/instability regions of values will permit us to draw inferences about the dynamics of the alliance formation process and to interpret the degree of stability and instability, something which is made possible by the clear-cut demarcation of the regions of stability and instability at the point zero.

Data Analysis

In testing Hypothesis 1 we must first determine the presence and the order of serial dependence in the autoregressive model (1). This is accomplished by examining the sampling distribution of the alliance formation data in terms of the sample autocorrelation (acf) and partial autocorrelation function (pacf). These functions are derived through the following relations

$$r_k = \frac{\sum_{k=1}^{n} (Y_t - \bar{Y}_t)\,(Y_{t-k} - \bar{Y}_t)}{\sqrt{\sum_{k=1}^{n} (Y_t - \bar{Y}_t)^2 \sum_{k=1}^{n} (Y_{t-k} - \bar{Y}_t)^2}} \qquad (6)$$

for *acf* (Box and Jenkins, 1970, p. 28) and

$$\phi_{kk} = \frac{r_k - \sum_{j=1}^{k-1} (\phi_{k-1,j})\,(r_{k-j})}{1 - \sum_{j=1}^{k-1} (\phi_{k-1,j})\,(r_{k-j})} \qquad (7)$$

for *pacf* (Box and Jenkins, 1970, p. 497).

From statistical theory it is known that a one-to-one correspondence exists between the theoretical autocorrelation functions and the autoregressive model: an autoregressive process is characterized by exponentially declining values in the *acf,* coupled with distinct nonzero values in the *pacf.* The number of distinct values in the *pacf* corresponds to the order of the autoregressive process after which all further values are zero. Thus, for instance, a second-order autoregressive process will have a series of exponentially declining *acf* values but two distinct nonzero *pacf* values. While we do not have access to the theoretical values, sample estimates from (6) and (7) give adequate approximation which we may examine (for probability error) in relation to their standard errors.[13] Our significance test calls for a confidence limit of two standard deviations. In other words, the sample *acf* values must be at least twice as large as their corresponding standard error values in order to be accepted as statistically significant—ensuring significance at approximately the 5 percent level. Estimated sample *acf* and *pacf* values for the nine series of data described previously are displayed in Tables 3, 4 and 5 along with their standard errors.

Inspection of the values reveals that the clearest stochastic pattern is contained in the existing alliance series for all systemic periods between 1815 and 1965. The values indicate first-order autoregressive (serial dependent) processes, evidenced by the exponentially declining *acf* and the single significant first-lag *pacf* value.

The *acf* and *pacf* values for the new and great power alliance formations in Tables 4 and 5, in contrast, reveal mixed results. Values for new and great power alliance formations in the tables strongly suggest random stochastic processes are present in the pre-World War I and interwar periods, as signified by the absence of any *acf* or *pacf* values twice as large as their standard errors. The post-1945 period, on the

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[13] The standard errors for the autocorrelation is (Box and Jenkins, 1970, p. 35)

$$SE(r_k) = \left\{\frac{1}{n}\left(1 + 2\sum_{i=1}^{k} r_i^2\right)\right\}^{1/2}$$

and the standard error for the partial autocorrelation value is (see Box and Jenkins, 1970, p. 65)

$$SE(\phi_{kk}) = \sqrt{\frac{1}{n}} \qquad \text{for } k > m$$

for m being the order of the corresponding autoregressive process.

other hand, displays significant serially dependent trends in the new and great power alliance series. The exponentially declining *acf* values imply autoregressive processes at work and the significant first *pacf* values further suggest first-order dynamics in the processes.

Fitting the nonrandom post-1945 new and great power alliance formation subseries by linear least squares produces the first-order discrete model of the form

$$Y_t = \phi Y_{t-1} + a_t \qquad (8)$$

which is prevalent in many political time series (see Li, 1976b), and a set of estimates which are given in Table 6, column 1.

Moreover, with respect to Hypothesis 2, we approach stability analysis by first transforming the discrete estimates given in Table 6 (column 1) into their continuous equivalents through relation (3). Since the characteristic roots are equivalent to the parameter values for first-order systems, no further computation for the roots is necessary as in higher-order cases. Thus, we may directly evaluate relation (4) in terms of the first-order parameter values and consider the solution[14]

Table 3. Annual Existing Alliance Autocorrelation and Partial Autocorrelation Functions

k	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Existing Alliances (1815–1914) $N = 99$														
r_k	.85	.76	.69	.65	.61	.54	.46	.42	.37	.34	.32	.25	.24	.21
SE	.10	.16	.19	.21	.23	.25	.26	.27	.28	.28	.28	.29	.29	.29
ϕ_{kk}	.85	.12	.07	.11	.04	–.12	–.04	.04	–.05	.04	.08	–.15	.11	–.03
SE	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10
Existing Alliances (1919–1939) $N = 21$														
r_k	.87	.72	.57	.43	.30	.16	–.00	–.11	–.16	–.19	–.26	–.36	–.43	–.45
SE	.22	.35	.41	.45	.47	.48	.48	.48	.48	.48	.49	.49	.50	.52
ϕ_{kk}	.87	–.13	–.10	–.04	–.08	–.16	–.16	.05	.13	–.07	–.22	–.28	–.00	.07
SE	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22
Existing Alliances (1945–1965) $N = 20$														
r_k	.83	.67	.51	.40	.27	.15	.01	–.19	–.36	–.47	–.43	–.39	–.34	–.33
SE	.22	.35	.41	.44	.45	.46	.47	.47	.47	.48	.50	.52	.54	.55
ϕ_{kk}	.83	–.10	–.05	.02	–.13	–.05	–.20	–.33	–.12	–.06	.33	.04	.00	–.06
SE	.22	.22	.22	.22	.22	.22.	.22	.22	.22	.22	.22	.22	.22	.22

r_k stands for autocorrelation function values; ϕ_{kk} stands for partial autocorrelation values and SE stands for standard error of the estimates (see equations 6 and 7).

Table 4. Annual New Alliance Autocorrelation and Partial Autocorrelation Functions

k	1	2	3	4	5	6	7	8	9	10	11	12	13	14
New Alliances (1815–1914) $N = 99$														
r_k	–.19	–.05	–.06	–.11	.06	–.00	.04	.04	–.09	.08	–.05	–.10	.11	–.06
SE	.10	.10	.10	.10	.11	.11	.11	.11	.11	.11	.11	.11	.11	.11
ϕ_{kk}	–.19	–.10	–.09	–.15	–.01	–.02	.03	.05	–.06	.07	–.01	–.11	.06	–.04
SE	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10
New Alliances (1919–1939) $N = 21$														
r_k	.02	–.17	–.20	–.32	–.04	.33	.14	–.07	.04	–.19	.03	.08	.02	–.12
SE	.22	.22	.22	.23	.25	.25	.27	.28	.28	.28	.28	.28	.29	.29
ϕ_{kk}	.02	–.17	–.20	–.36	–.17	.17	.01	–.13	.13	–.03	.15	–.01	.02	–.12
SE	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22
New Alliances (1946–1965) $N = 20$														
r_k	.47	.21	.02	–.09	.06	.22	.14	–.05	–.16	–.20	–.18	–.03	.08	–.16
SE	.22	.27	.28	.28	.28	.28	.29	.29	.29	.30	.30	.31	.31	.31
ϕ_{kk}	.47	–.01	–.09	–.09	.21	.19	–.10	–.23	–.03	.02	–.11	–.01	.13	–.29
SE	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22

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[14] The system in this case is identical to a homogeneous equation

$$DY(t) + \alpha_0 Y(t) = 0$$

$$Y(t) = \int_{-\infty}^{t} e^{-\alpha(t-v)}\, Z(v)\, dv. \qquad (9)$$

From the above relation it is apparent that the dynamics of $Y(t)$ in (9) primarily depends on the value of α (or equivalently on u). When α is greater than zero, $\alpha > 0$, the associated (alliance formation) processes are characterized by dampening forces and strong resistance to change from the equilibrium position (which is $E(Z(t)) = 0$ as noted before). Accordingly, as α increases in size, resistance to change intensifies and disturbances induced by the transformed serial dependent effect in $Z(v)$ diminish quickly over time. Otherwise, when α is lesser than or equal to zero, $\alpha \leqslant 0$, the associated process would either explode away from equilibrium or drift aimlessly through multiple equilibrium points. Thus, as α decreases in size, the effect of serial dependence simply magnifies over time.[15]

Interpretation and Conclusion

Overall, evidence from this analysis seems to have satisfied the test criteria T_1 and T_2 proposed as corollary to the principal hypotheses requiring evidence of serial dependence and stability in the 1945–1965 period and none in the 1815–1914 and the 1919–1939 periods.

In conjunction, the findings in Tables 3 and 6 reveal the existing alliance series to be an unstable first-order process. Initial model identification reveals first-order serial dependence for all subseries between 1815 and 1965 and the near-zero values of the derivative continuous estimates further imply that the processes are of the unstable variety that drifts aimlessly through time.

The most plausible interpretation for these unstable results is that they reveal not so much the true dynamics of the alliance formation series as the artifacts of data aggregation. Since the existing alliance formation series are constructed by adding annual new alliance frequencies to pre-existing alliances, the result is an index of cumulative frequencies that invariably rises over time. This rising trend is what largely contributes to the unstable parameter estimates. There is of course no reason to confuse this trend with growth in international system size, since that factor has already been controlled. Rather, it reflects the fact that the number of new alliances has cumulatively increased over time.

In keeping with the above observation, it is only natural that new and great power alliance formation series should evince clearer patterns of stochastic dynamics. According to the parameter values shown on Table 6, the new and great power alliance formation subseries for the 1815–1914 and 1919–1939 periods possess essentially random dynamics that exhibit drifting patterns over time, while the 1945–1965

Table 5. Annual Great Power Alliance Autocorrelation and Partial Autocorrelation Functions

k	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Great Power Alliances (1815–1914) $N = 99$														
r_k	–.07	.05	–.08	.07	–.01	.11	.04	–.02	–.09	.14	–.01	–.09	.09	–.03
SE	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.11
ϕ_{kk}	–.07	.04	–.07	.06	.01	.10	.06	–.02	–.09	.12	.01	–.12	.10	–.03
SE	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10	.10
Great Power Alliances (1919–1939) $N = 21$														
r_k	.15	.18	.38	–.03	–.07	.13	.09	–.16	.04	–.09	–.22	–.13	–.15	–.19
SE	.22	.22	.23	.26	.26	.26	.26	.26	.27	.27	.27	.28	.28	.29
ϕ_{kk}	.15	.16	.35	–.16	–.19	.07	.21	–.18	–.12	–.16	.01	–.07	–.10	–.09
SE	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22
Great Power Alliances (1946–1965) $N = 20$														
r_k	.48	.27	.06	.10	.15	.13	.12	–.14	–.10	–.11	–.12	–.13	–.14	–.15
SE	.20	.25	.26	.26	.26	.27	.27	.27	.27	.27	.28	.28	.28	.28
ϕ_{kk}	.48	.05	–.12	.14	.10	–.01	.05	–.29	.07	–.01	–.17	–.00	–.02	–.08
SE	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22	.22

(where $DY(t) = \dfrac{dY(t)}{dt}$) with the initial condition $C(0) = 1$.

[15] These patterns are verified by integrating (9) with the value of α being greater than, lesser than or equal to zero.

period clearly contains first order serial dependence which appears basically stable by virtue of the magnitude of the ϕ or α estimates.

So, apart from the existing alliance formation series which we deem to be seriously compromised by the statistical artifact of aggregation, patterns of stochastic dynamics observed in the new and great power alliance formation series essentially affirm the test criteria embodied in Hypotheses 1 and 2: that serial dependence and stable dynamics existed in the 1945–1965 period characterized by a B/T structure whereas no significant dependence effect, apart from an unstable dynamic, prevailed in the 1815–1914 and 1919–1939 periods characterized by a M/L structure. In short, what has been corroborated, subject of course to the limitations of a rather small N size (3 system periods), is the observation that alliance formation behavior in a B/T system is likely to be characterized by serial dependence and stable dynamics and that in a M/L system, it is likely to be characterized by random and unstable dynamics.

The success of our two hypotheses is highly suggestive. First, the very distinctiveness of the findings is extremely helpful in sorting out the ambiguity left by earlier analyses using the Poisson model. In keeping with earlier findings, we have reconfirmed the emerging consensus

Table 6. Discrete and Continuous Parameter Estimates, for Serial Dependence, Characteristic Roots and Dynamics Characterization

Alliance Categories	Discrete Estimates (ϕ)	Continuous* Estimates and (α) Roots	Dynamic Characteristics
Existing Alliances			
1815–1914	.976	(**) .040 (**)	Unstable
1919–1939	1.000	(**) .000 (**)	Unstable
1945–1965	.951	(**) .049 (**)	Unstable
New Alliances			
1815–1914	.000	(**) .000 (**)	Unstable
1919–1939	.000	(**) .000 (**)	Unstable
1945–1965	.488	(1.588) .717 (.153)	Stable
Great Power Alliances			
1815–1914	.000	(**) .000 (**)	Unstable
1919–1939	.000	(**) .000 (**)	Unstable
1945–1965	.461	(1.695) .773 (.148)	Stable

*Note for a first order model, the coefficient and its characteristic root are the same; they differ in value for second and higher order models. The values in parentheses over and below each estimate are the corresponding confidence intervals.

**These confidence intervals are either extremely large (.1 X 10^{31} and –.1 X 10^{31}), suggesting rejection of the parameter estimate as statistically insignificant or they are astriding the zero values suggesting statistical insignificance, i.e., for the 1945–1965 Existing Alliance series the upper and lower confidences for .049 are .294 and –.193 respectively.

that nineteenth-century alliance formation behavior was basically random. But, in contrast to the rather inconclusive earlier results, we have observed that the randomness of the nineteenth-century period does indeed persist into the twentieth century until the eve of World War II. This should help in resolving some of the contradictions which emerge when one compares the somewhat conflicting analytical outcomes of Job (1973), Siverson and Duncan (1973), and McGowan and Rood (1975).

Second, and as implied by our first hypothesis, the alliance formation dynamics of the post-1945 period do seem to require a broader explanatory framework than the balance of power thesis articulated by McGowan and Rood (1975). While we do not wish to engage in the debate over whether a balance of power process actually operated in the nineteenth century, we feel that we are on safe ground when we assert that the types of international systems which have been discussed by theorists of international politics as subject to balance of power politics are systems which can also be described as having a M/L structure. Thus what we have done is to subsume the balance of power concept's usual emphasis on the necessity of multiple actors and alliance flexibility under our own emphasis on capability distributions and the tightness of bloc structures. By doing so, we are in a position to avoid relying upon some of the more questionable premises of balance of power arguments which are, at the very least, restricted to certain periods of history and not applicable to other periods. Instead, our construct permits us to concentrate on attributes which are applicable to any international system at any time. The prediction and finding of serial dependence in the 1945–1965 period, a period not characterized by a multipolar/loosely knit structure, strongly underscore this point. By our broader construct we are able to account for the absence of serial dependence in the periods between 1815 and 1939, as well as for the presence of serial dependence in the post-1945 period. In this respect, we contend that a focus on capability distributions and tightness of bloc structure is more powerful than the type of balance of power models employed by McGowan and Rood in a similar context and by a number of other analysts in different contexts.

Third, our findings also reaffirm the idea that alliance formation behavior is indeed responsive to structural attributes of the international system.[16] To be sure, empirical support for this contention is available (e.g., Holsti, Hopmann, and Sullivan, 1973, pp. 79–83); but we have attempted to go beyond the vague alliance structural linkage to more concrete terms specifying which structural attributes are important. Given the awkwardness of our measures of capability distributions and tightness of bloc structures over the 1815–1965 period, we cannot be as precise about the nature of the linkage as we might otherwise wish. Nevertheless, the evidence appears to suggest that the serial dependence and stability of the alliance formation processes are influenced by the type of capability distribution and tightness of bloc structures which characterize the international system at particular points in time.

Finally, with respect to our second hypothesis, a major finding of this study is the stability of serial dependence in the post-1945 alliance formation process and of its absence in the periods prior to 1945. The period in which we find process stability is also characterized by an international system with a B/T structure. The concurrence of these attributes raises interesting implications for the relationship between alliance formation processes and the probability of war.

Earlier in this article, we argued that the diminished interaction opportunities associated with a B/T structure could be expected to contribute to stabilizing the alliance formation process by enhancing the resistance to ever-escalating alliance proliferation as in a Richardson arms race process. The structure of the situation tends to reduce actor flexibility in choosing new alliance partners or discarding old partners. What evolves is something like an alliance stalemate between the two major blocs. Consequently, inflexibility appears to be associated with stability of alliance formation process while the flexibility of systems with a M/L structure appears to be connected to instability.[17] If this is true–inflexibility (flexibility) is related to stability (instability)—it is quite possible that the role of alliance formation processes in relation to war is more complex than previously thought.

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[16] We should note, however, that it is difficult from a mathematical standpoint (in the absence of serial dependence) to separate the effect of random fluctuation from the homogeneous instability observed for the periods between 1815 and 1939. It is possible that both randomness and instability have occurred. It is also possible that the observed homogeneous instability is simply a statistical artifact resulting from the random distribution itself.

[17] Still, it must be pointed out that we cannot totally rule out certain plausible rival hypotheses, for the capability distribution and the tightness of bloc structure are not the only variables which underwent change in the aftermath of World War II. An obvious contender for attention is the introduction of nuclear

Schroeder (1976), for instance, has argued that an important function of all alliances, in addition to the previously mentioned capability aggregation, is to restrain and control the actions of alliance partners and opponents alike. In this sense, alliances function as systemic instruments of political management and order. As such, Schroeder (1976, p. 256) may be correct in suggesting that peace can be promoted by having states locked tightly into alliances which function as pacts of mutual restraint. Similarly, the alliance inflexibility and process stability found in the B/T structure of 1945–1965 may well have had a restraining influence on the likelihood of interbloc violence. Certainly, we might expect this restraining function of alliances to be more easily accomplished in a system dominated by two superpowers than in one subject to the competitions and rivalries of multiple—and hence more difficult to coordinate and restrain (contrary to the invisible hand argument)—great powers. Conversely, we might also expect alliance flexibility and process instability linked to M/L structures, to lead to a higher frequency of minor outbreaks of violence between the major actors. Such clashes can occasionally escalate into major confrontations as in the Austro-Serbian dispute and World War I.

Needless to say, this assertion of a relationship between the stability of the alliance formation process, alliance inflexibility, and the reduced likelihood of war is speculative, for we have gone well beyond the inherent limitations of our present data analysis. We have done so only in the hope of clarifying what we believe to be the ultimate political implications of the stochastic nature of alliance formation behavior. Since our formulation explicitly contradicts other arguments to the contrary (e.g., Deutsch and Singer, 1964), only further research empirically incorporating the variable of war can test the accuracy of the above speculation.

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weapons. Since this phenomenon coincides with the presence of B/T structure, it is extremely difficult to separate the impact of new weaponry systems from the structural conditions found in the post-1945 period. This is all the more true since the continuing development of nuclear weaponry can be seen as both a source as well as a product of the B/T structure. The global reach and the high proportion of the world's military capabilities possessed by the two superpowers are based to some extent on the advantages and costs of nuclear weapons just as the potential destructiveness of the available weaponry bears a complicated (both positive and negative) relationship to East-West threat perceptions (which are presumably related to the tightness of the bloc structure). Yet it is also true that the control of these same weapons has been an important issue involved in bloc disintegration processes. Unfortunately, all of these considerations only serve to underscore the difficulty of controlling for the possible influence of such an intervening variable or alternative explanation, at least with the data base examined in this article. Hence, our interpretations need to be evaluated with the customary tentativeness of less than complete investigations.

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A Theory of Political Districting*

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Fair districting requires more than compact, contiguous equal-sized districts; namely, sets of districts *should also possess certain features. Specifically, they should be neutral (treat all parties alike) and responsive to changes in votes. In order to establish the extent to which these goals can be achieved, we give a precise definition to the concept of neutrality and expand the notion of responsiveness into three characteristics: the range in actual votes over which a districting plan is responsive; the degree of responsiveness in the vicinity of the "normal vote" (i.e., competitiveness); and the constancy of the swing ratio (i.e., the rate at which vote changes yield seat changes) over a range of votes. We show that while all possible values for these features are readily attainable when considered individually, certain combinations of values cannot be achieved. Finally, we identify the nature of the compromises required and the properties that the compromises possess, and show the kinds of trade-offs that result in reasonably fair districting plans.*

In this article we consider the task of dividing a political unit into legislative districts. The most frequently considered features of size, compactness, and contiguity have been extensively dealt with elsewhere and are not our concern here. Rather, we are concerned with four features of districting plans which provide a useful characterization of the way in which the partisan allocation of legislative seats responds to changing vote totals. After defining the features and showing how they can be achieved individually, we show that certain combinations of these characteristics—in fact those which are probably the most desirable combinations—cannot be achieved. Having shown that trade-offs are a virtual necessity, we then turn to the more difficult task of prescribing standards for fair districting.

Four Characteristics of Districting Plans

Our ultimate goal is the normative one of proposing standards against which the quality of districting plans for political units can be judged. Even apart from the widely accepted standards of compactness, contiguity, and equal size, a large number of other standards could be established. For example, "accessibility" within districts could be maximized (Taylor, 1973, p. 948), or the degree of homogeneity of the districts could be maximized or minimized (Mayhew, 1971, pp. 270–73). Here we limit our investigation to characteristics which indicate the responsiveness of an entire districting scheme to changing vote totals. As Tufte (1973) has pointed out, many districting schemes can be devised which satisfy criteria such as compactness, equality of size, and so on, but which many observers would still regard as unfair because large shifts in votes result in few or no seats changing hands. If we accept the premise that seats in a representative body should change in some specified way as vote totals change, then it is apparent that there is a need to incorporate the partisan division of the vote into criteria for fair districting.[1]

We suggest that four parameters (or characteristics) can be used for this purpose:

(1) *Neutrality.* A districting plan which treats all parties alike in allocating seats per given vote totals is said to be neutral. Stated more formally, a districting plan is neutral when v percent of the popular vote results in s percent of the seats, and this holds for all parties and all vote percentages. Although it would be useful to have a simple measure of the degree to which a given districting scheme departs from neutrality, no such measure is easily defined. Therefore, for present purposes this is a 0–1 attribute: A districting plan is neutral (1) or it is not (0).

(2) *Range of Responsiveness.* The range of responsiveness (RR) of a districting plan is defined as the percentage range of the total popular vote (for the entire political unit) over

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[1] There is a serious question as to whether the courts would enforce the use of the partisan division of the vote as a districting criterion. (Legislators already rely on the party vote, but not in an attempt to devise fair districting schemes.) Although the answer is not at all clear, legal scholars do not reject the possibility (Dixon, 1968, p. vi, Ch. 13; Baker, 1971).

which seats change from one party to the other. Specifically, the low end of the RR is the minimum percentage of the total vote required for a party to win at least one seat (or one-half seat as interpreted in n. 9), while the upper end is the minimum percentage of the total vote required to win all of the seats. Obviously the RR can be any part of the 0–100 percent vote range.

(3) *Constant Swing Ratio.* The swing ratio (SR) of a districting plan is defined as the rate at which a party gains seats per unit increment in votes. When this rate is identical for all vote percentage points over a specified range, the SR is said to be constant (over that range). As with the neutrality parameter, it would be useful to have some measure of degree of departure from a constant SR. However, here too we use a 0–1 parameter, with the SR constant (1) or not (0).

(4) *Competitiveness.* The competitiveness of a districting plan is defined as the percentage of districts in which the "normal" vote (defined below) is within some fixed distance of 50 percent.[2]

All four of these parameters are important considerations in evaluating the adequacy of a given districting plan. The neutrality condition is a simple criterion of fairness since it merely requires that parties be treated the same way in relating seat totals to vote totals. In addition, it is closely related to the concept of "manufactured majorities," in which a party wins a majority of the seats with less than half of the votes (Rae, 1971, pp. 74–76). The range of responsiveness is important because it helps determine whether or not a party survives a landslide election or a series of such elections (Mayhew, 1971, p. 266; Campbell, et al., 1960, p. 553). Under some circumstances (especially in proportional representation systems) the lower end of the RR is particularly controversial since it represents the "threshold of representation" (Rokkan, 1968, pp. 12–25; Rae, 1971, passim). The swing ratio over a range of votes is important because it describes the rate at which changes in the vote are translated into changes in legislative seats. Moreover, districting plans may be sufficiently insensitive to vote changes that the responsiveness of legislators (i.e., constituency accountability) becomes problematic (Tufte, 1973). Finally, the competitiveness of individual districts and of sets of districts is important because it is said to be related to a variety of political phenomena such as congressional voting behavior (Fiorina, 1974; Mayhew, 1971, pp. 255–69) and the policy output of state legislatures (e.g., Cnudde and McCrone, 1969).

In addition to the fact that each of these four characteristics is important in its own right, they also possess important interdependencies. To see an illustration of this point, consider the competitiveness parameter. A districting plan might be judged acceptable in terms of competition if, for example, all districts had a "normal" vote of 55 percent favoring one party. If, however, one imposed a wide range of responsiveness, or required a constant SR over the votes interval 40 percent to 60 percent, a very different kind of districting plan would be required. Indeed, the changes brought about by considering several characteristics simultaneously highlights one of the major points we make; namely, that *all four* characteristics must be considered in evaluating the fairness of a districting plan.[3]

In order to understand better the properties of each of these four characteristics, as well as the ramifications of their interdependencies, we need to work with a well-defined model. The model we use is described in the next section.

The Model

Let us consider the task of dividing a political unit (e.g., a state) into electoral districts. We assume the following:

1. There are two political parties (which we call Republican and Democrat).
2. Districts are of equal size.
3. Clustering of the population (such as most Democrats living in one geographical area) does not prevent achievement of desired districting.
4. A "normal" vote for the political unit of interest (symbolized TNV, for total normal vote) describes the expected partisan division of the vote in future elections for the office in question. Each district with-

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[2] None of the currently available definitions of competitiveness, including the one used here, is entirely satisfactory. As we will shortly make clear, however, the problems associated with these definitions are largely overcome by considering all four of the characteristics we have defined.

[3] Implicit throughout our discussions are the parameter values we tentatively favor–neutrality (=1), a wide RR, a constant (or nearly constant) SR over relatively wide ranges, and a fairly high degree of competitiveness. Even if one favors widely different values, however, we believe that these parameters are important features by which to judge the quality of a districting plan.

in the political unit is also characterized by a normal vote (symbolized DNV_i, where i is an index referencing a particular district).

5. There are no constituency effects in the vote.

Assumption 1 is a simplification for convenience and can be eliminated by developing models for multiparty systems. Assumption 2 is a relatively innocuous assumption in that minor variations from equal size will not substantially change the results of our analysis. Moreover, the essential results presented below hold for virtually all district sizes, although the specific details do differ. Assumption 3 is a simplification that we feel must be incorporated in our model because it is almost impossible to deal abstractly and yet systematically with real-world population clustering. Furthermore, part of our objective is to show that even when real-world problems such as this are assumed away, it is still impossible to formulate districting plans with certain combinations of features that many would likely view as desirable. If one were in fact attempting to implement our proposed compromises, one would simply have to attempt to do the best possible in the face of population clustering.

Assumption 4 states that we know, or can estimate, a "normal" or expected vote in the total political unit and within each electoral district. Specifically, if DNV_i represents the proportion of voters in district i who "normally" vote Democratic, then the TNV is simply a weighted mean of the individual DNV's, where the weights employed depend on the size of each district. Here, since we assume that all districts are equally sized, $TNV = \frac{1}{n} \Sigma \; DNV_i$, where n is the number of districts. For future reference it should be noted that, unless otherwise stated, all references to proportions (or percentages) will be with respect to votes and seats won by the Democratic party.

For our purposes, it is immaterial how one might go about measuring or determining a normal vote. Our only concern is that it can be used as a base-line for predicting the expected division of the party vote in subsequent elections. Past voting results are probably the most frequently used mechanism for estimating a normal vote, but registration figures, survey results, or some other means (such as forecasting future election results on the basis of projected demographic changes in the electoral districts) could also be used. We would argue that incumbent legislators frequently make assessments of this sort in an attempt to anticipate the partisan consequences of proposed districting plans. Thus, our assumption simply extends what we view as established practice by requiring that a specific estimate of future votes in a district be available rather than a general characterization of the district as, for example, "safe Democratic."

Assumption 5 states that there are no constituency effects in the vote. We ignore constituency effects simply because there is little one can do to anticipate them and take them into account. For example, it may be thought that a particular district is safely Democratic. But, if an especially attractive Republican candidate were to seek election, or if a particularly unattractive Democratic candidate were to be nominated, or if an unexpected issue arose which was favorable to the Republicans, or if the constituency changed rapidly because of migration, the seat might be closely contested. Events of this sort will obviously influence the actual competitiveness of electoral districts and, of course, will also affect the responsiveness of a given districting plan to changes in the vote total. Yet for the most part constituency-specific factors can be either roughly taken into account when estimating the normal vote or are effects which simply cannot be foreseen at the time of districting. In either case, it seems safe to ignore them in our model.

Given these assumptions, our model can be graphically represented as a distribution of seats (characterized by their district normal votes) and a related seats-votes curve. An example is shown in Figure 1, where a political unit with TNV = 50% is divided into 100 districts with DNV's uniformly distributed between zero and 100 percent.[4] The seats-votes curve depicted shows the expected proportion of the seats the Democrats will win for each percentage of the total vote they receive. Figure 1 represents an extreme instance of proportional representation: at each point on the curve the Democrats expect to win exactly as many seats as they do votes.

Before proceeding, we need to clarify the way in which deviations in the total *actual* vote (TAV) from the total *normal* vote (TNV) are

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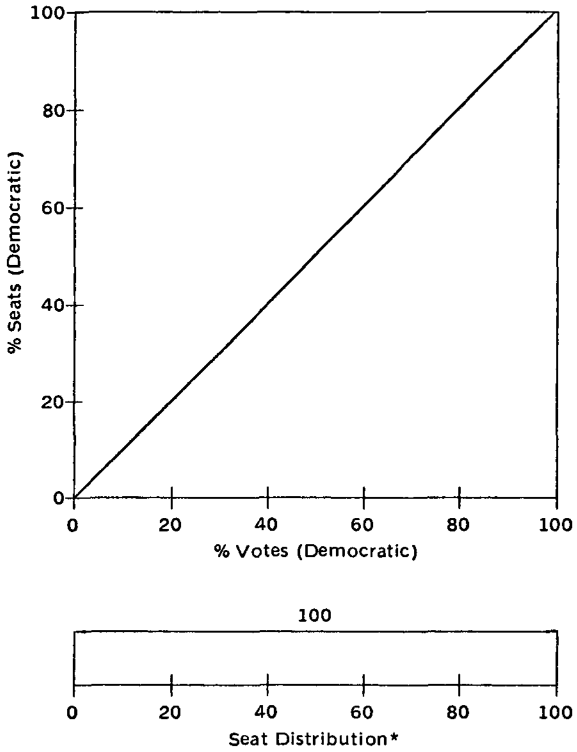
[4] See the Appendix for the precise way in which the DNV's are distributed. Except for Figure 8, all of our figures assume 100 districts. Our analysis is best illustrated with a fairly large number of districts, and 100 is convenient since each district represents exactly one percent of the seats. Small numbers of districts do not alter the basic results of our analysis, but they do make it more difficult to achieve satisfactory results. This is taken up below.



*Districts (i.e., DNV's) are uniformly distributed across the entire 0—100% Votes interval. The Appendix contains the exact specification of all seat distributions displayed in the figures.

Figure 1. Hypothetical Seat Distribution and Related Seats-Votes Graph

distributed among the districts. Consider the situation in Figure 1. In accordance with assumption 5, the district actual vote (DAV) deviates uniformly from the DNV in all districts. Therefore, since it is assumed that no constituency effects are present, if the Democratic vote percentage were to drop, say, 10 percent below normal in a given election, it must be 10 percent below normal in each and every district. Now suppose that the TAV is 40 percent Democrat. Since districts cannot obtain less than zero percent of the vote, there are districts in which the DAV cannot be 10 percent below the DNV (i.e., all districts in which DNV $<$ 10%). Consequently (since by supposition the TAV = 40%), the vote in other districts must necessarily decline by more than 10 percent. It is possible, but unnecessarily complicated, to figure out what that decline must be. It turns out that for a TAV of 40 percent, the 89 most Democratic districts would get 10.61 percent less than their normal vote, so that the DAV's would be .39 percent, 1.39 percent, and so on.

A simpler procedure, and the one we use, is to interpret a difference between the TNV and TAV as meaning that this same difference exists between the DNV and DAV in all districts, subject to the limits of zero and 100 percent. Thus, for example, if TNV–TAV = 10%, the vote in each district is DNV – 10% if the DNV $\geqslant$ 10%, and 0% if the DNV $<$ 10%. This simplification permits us to speak of the TAV as some percentage, say 40 percent, while our interpretation is that the actual vote in each district is (TNV–TAV)% below the DNV, subject to the limits of zero and 100 percent. (Algebraically, DAV = DNV – (TNV – TAV), with DAV constrained to be between zero and 100 percent, inclusive.) In practice, this interpretation is likely to cause few problems since electoral districts are not usually designed to have extremely large or extremely small DNV's, and since actual votes do not usually deviate too far from the normal vote.

Having shown how the actual vote is distributed among the districts, we now define the swing ratio and show how it is calculated. The swing ratio (SR) is defined as the percent change in seats corresponding to a one percent change in votes. Let us show how to determine this rate of change with a specific example. Consider once again Figure 1, and suppose that the $\text{TAV} = 40\%$ (meaning, as noted, that the actual vote in each district is either 10 percent less than the district normal vote, or zero). Given this situation, the Democrats would win all seats having a $\text{DNV} > 60\%$ (i.e., 40 seats), since they would receive over 50 percent of the popular vote in each of these districts. If the Democratic vote were to increase by one percent to a $\text{TAV} = 41\%$, the Democrats would win all seats with a $\text{DNV} > 59\%$, for a one-seat gain.[5] Thus, in going from 40 to 41 percent, a one-percent increase in the vote total leads to a one-percent increase in seats, so the swing ratio (SR) equals 1.0. Note that this same rate of seat changes to vote changes is true for all percentages in Figure 1, so $\text{SR} = 1.0$ for the entire 0–100 percent range.

Since seats cannot be split fractionally between the parties,[6] it will sometimes be useful to calculate an average SR over a range of votes. For example, if 25 districts are uniformly spread over 15 percentage points, one-third of the time a gain of one percent in votes will yield a gain of only one seat while two-thirds of the time a one-percent gain in votes will yield two seats. What is important, however, is that the average gain over that range is $25/15 = 1.67$ seats per one-percent gain in votes, and that the uniform distribution of seats yields the closest possible approximation of that seats-votes relationship. (That is, the average gain is equivalent to the linear regression line over that range.) In such situations, the average gain is the value reported for the SR.[7]

Achieving Individual Parameter Values

Using the model just described, it is possible to design districting plans in which the four responsiveness parameters take on specified values. We show how this can be done for each parameter in turn.

Neutrality. A districting plan is neutral when the distribution of district normal votes around the total normal vote is symmetric.[8] This can be easily demonstrated. First, for any arbitrary TNV in the interval zero to 100 percent, consider the case where $\text{TAV} = 50\%$ Democratic. By definition there exists an x such that $\text{TNV} - x = \text{TAV} = 50\%$, where $-50\% \leqslant x \leqslant 50\%$. It follows that the Democrats would win all seats for which $\text{DNV} > (50 + x)\%$ since in these districts the actual vote (DAV) would be more than 50 percent Democratic. Since $(50 + x)\% = \text{TNV}$, and since the DNV's are distributed symmetrically about the TNV, the Democrats would win exactly one-half of the seats.[9] Hence the system is neutral when the actual vote is 50 percent Democratic. Now suppose that the Democratic vote *increased* from 50 percent by some number y, where $0\% < y \leqslant 50\%$. The Democrats would take from the Republicans the following seats (in addition to maintaining those for which the $\text{DNV} > \text{TNV}$):

$$(1)\ \left\{\begin{array}{l}\text{half of the seats for which DNV} = \\ \quad \text{TNV (having already won half of} \\ \quad \text{these seats);} \\ \text{half of the seats for which DNV} = \\ \quad \text{TNV} - y\text{, and} \\ \text{all seats for which TNV} - y < \text{DNV} < \\ \quad \text{TNV.}\end{array}\right.$$

Conversely, if the Democratic vote *decreased* from 50 percent by y, the Democrats would lose to the Republicans the following seats (in addition to those seats already lost for which the DNV $<$ TNV):

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[5] As noted in the Appendix, we give DNV's values which are ϵ above or below an integer value. Thus there is a $\text{DNV} = (60 + \epsilon)\%$ Democratic and another with $\text{DNV} = (59 + \epsilon)\%$ Democratic. When $\text{TAV} = 40\%$ Democratic, the former district will be won by the Democrats (with a vote of $(50 + \epsilon)\%$) but the latter will be lost (with a vote of $(49 + \epsilon)\%$). When $\text{TAV} = 41\%$, the latter district will also be won by the Democrats. Hence the one seat gain.

[6] A minor exception to this can occur in our model because we have to deal with the possibility that a vote exactly equals 50 percent. See n. 9 below.

[7] This feature is not peculiar to our model. No matter how calculated, any swing ratio that is not integer-valued involves some smoothing since a one-percent increase in votes cannot bring about a fractional gain in seats. Most calculations of the SR involve regressing seats on votes over some range of votes. If desired, the precise SR's at each percentage of the vote can be determined from the distributions given in the Appendix.

[8] Symmetry is actually a necessary and sufficient condition for neutrality. We only prove the sufficiency condition.

[9] We assume that when the actual vote in a district is exactly 50 percent Democratic, the seat(s) is (are) split equally between the parties. This is unrealistic, of course, as it results in parties winning "half seats," but it seems much more appropriate than alternative rules (e.g., arbitrarily assigning the seat(s) to one party). One can think of a half seat as a .50 probability that the seat would be won.

$$(2)\quad \begin{cases} \text{half of the seats for which DNV = TNV (having already lost half of these seats);} \\ \text{half of the seats for which DNV = TNV} + y\text{, and} \\ \text{all seats for which TNV} < \text{DNV} < \text{TNV} + y. \end{cases}$$

Since by stipulation the distribution of DNV's is symmetric about the TNV, the same number of seats satisfies (1) and (2). Hence, the system is neutral throughout the entire 0–100 percent range.

Note that neutrality is achievable regardless of the value of the TNV, the magnitude of the range of responsiveness, and the shape of the distribution of DNV's (as long as the distribution is symmetric about the TNV). Furthermore, neutrality is a property that is unaffected by small numbers of districts, non-constant swing ratios, and other factors that are considered below.

Range of Responsiveness. The range of responsiveness is a function of the greatest deviation of the district normal votes above and below the total normal vote. More precisely, let the least Democratic DNV be TNV$-x$, and the most Democratic DNV be TNV $+ y$. Then the RR is $(50 - y)\%$ Democratic (or 0% if $y > 50\%$) to $(50 + \epsilon + x)\%$ Democratic (or 100% if $x > (50 - \epsilon)\%$). To prove this, assume that in the most Democratic district DNV = TNV $+ y$. The difference between this DNV and 50 percent is TNV $+ y - 50\%$, which may be a negative value. Hence, the actual vote in the most Democratic district can be as low as TNV $+ y - 50\%$ below the TNV, and the district would still go (one-half) Democratic. Since $(\text{TNV} - (\text{TNV} + y - 50))\% = (50 - y)\%$, the system will be responsive when the total actual Democratic vote reaches $(50 - y)\%$ and is unresponsive below that value. A similar argument using the least Democratic district shows that the Democrats would win all the seats when that district's actual vote is $(50 + \epsilon - (\text{TNV} - x))\%$ above the TNV. Since $(50 + \epsilon - (\text{TNV} - x) + \text{TNV})\% = (50 + \epsilon + x)\%$, the system will be *unresponsive* after the total actual Democratic vote reaches $(50 + \epsilon + x)\%$.

Note that the RR is restricted to less than the 0–100 percent range if TNV $\neq$ 50%. For example, if TNV = 65%, the RR can at most be from 15 to 100 percent; if TNV = 80%, the RR is at most 30 to 100 percent.[10] If TNV $\neq$ 50% and the number of districts is small, the RR may be further restricted since it may not be possible to have districts with both zero percent and 100 percent DNV's. Nevertheless, within these constraints, any desired RR can be achieved simply by creating districts with DNV's at appropriate distances above and below the TNV.

Constant Swing Ratios. The swing ratio will be constant over the range $(50 - x)\%$ to $(50 + x)\%$ if the DNV's are distributed uniformly from (TNV $- x$) to (TNV $+ x$).[11] As noted above, if the most Democratic district has a DNV = TNV $+ x$ Democratic, the Democrats will begin winning seats when the TAV = $(50 - x)\%$. Then, because of the uniform distribution of DNV's, every one-percent increase in the vote will yield a constant increase in the number of seats (i.e., the average gain per one-percent increase in the vote will be constant) until all seats are won. Examples are given in Figure 2 where x = 10% and 25%. Note that a uniform swing ratio over the entire RR guarantees neutrality.

Competitiveness. Since it is often assumed that competitiveness is a desired property, we show that maximizing the number of competitive districts can be accomplished in a straightforward manner. If one instead wished to minimize the number of competitive districts, or fix the number at some specified level, this could be accomplished in a similar fashion. To begin with, if the TNV falls within what is considered the competitive range, competition can be maximized by simply making DNV_i = TNV for all districts. On the other hand, if the TNV is outside the competitive range, there exists an upper maximum percentage (less than 100 percent) on the number of districts that can be made competitive. It can be shown that this percentage is dependent on the TNV, the number of districts, and the level at which a DNV is regarded as competitive.

Rather than showing a general (and needlessly complex) solution, however, let us simply stipulate that competitive districts are those districts having a DNV of 45 to 55 percent, and suppose that the TNV is outside this range on

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[10] Since ϵ can be arbitrarily small anyway, we will simply drop it where there is no danger of ambiguity.

[11] Constant swing ratios over other ranges (i.e., not symmetric about 50 percent) are constructed in a similar manner. Again we prove only a sufficiency condition.

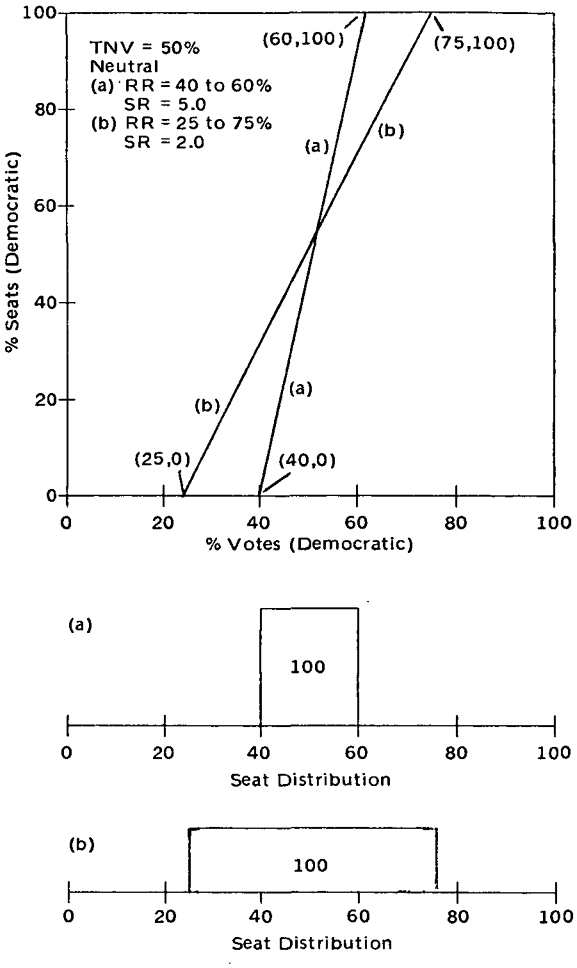

Figure 2. Examples of Uniform Swing Ratios with Different Ranges of Responsiveness

the Democratic side. The number of competitive districts can be maximized if we allow the competitive districts to be at the extreme pro-Democratic edge of the competitive range, i.e., at 55 percent Democratic. Therefore one solution to the problem of maximizing the number of competitive districts (or equivalently, minimizing the number of noncompetitive districts) is to solve the following constrained maximization (minimization) problem, viz., maximize a in:

$$55a + x[b] = n \times \text{TNV}, \text{ subject to} \quad (1)$$

$$a + [b] = n \text{ and} \quad (2)$$

$$\text{TNV} < x \leqslant 100\%, \quad (3)$$

where a = the number of competitive districts; $[b]$ = the smallest integer satisfying the equations (i.e., the number of noncompetitive districts), and x = DNV (in percent Democratic) of the noncompetitive districts.

To illustrate the solution suppose $n = 10$ and TNV = 60%. From (1),

$$55a + x[b] = 600. \quad (4)$$

Solving (2) for a and substituting into (4), yields

$$[b] = \frac{50}{x-55}. \quad (5)$$

Solving (5) subject to the constraint (3) yields

$[b] = 2$ and $x = 80\%$, and from constraint (2), $a = 8$. In other words, by making 2 districts 80 percent Democratic, 8 of the 10 districts can be made competitive (with DNV's = 55%).

It should be noted that this solution is not always unique, and may not be entirely satisfactory. Moreover, if the TNV is quite far from 50 percent, no amount of juggling (with equal-sized districts) can make a large proportion of the districts competitive. Nevertheless, by itself, maximizing the number of competitive districts is not a difficult task.

Impossibility Results

Thus far we have defined four parameters which describe the way in which sets of district seats respond to changing vote totals. Moreover, we have shown that certain values for these parameters can be easily achieved when considered individually: namely, neutrality, any (feasible) RR, a constant SR, and maximum competitiveness can all be obtained with ease. Simple as it is to achieve these values individually, however, it may be impossible to attain certain *combinations* of these parameter values in a given districting plan.

Whether or not a particular combination of parameter values can be achieved depends on the precise values specified for the parameters, on the TNV, and on the number of districts. If, for example, TNV = 50%, there is a large number of districts and one is satisfied with a districting plan that is neutral, has an RR from 25 to 75 percent, has a constant SR, and has at least 40 percent of the DNV's in the range (50 ± 10)%, then such a districting plan can be designed with all of these features. Figure 2(b) is such a plan for 100 districts. However, if it were instead required that half of the districts have DNV's in the range (50 ± 10)%, or required that the RR be 20 to 80 percent, or if the TNV = 67%, then no plan can be designed that satisfies all of these values.

It is possible, by means of a linear program, to express the interdependencies among the four parameters, for a given TNV and a fixed number of districts. Such a program would show, in a general way, the region of feasible solutions and the trade-offs among the parameter values for political units with a given number of districts and with a specified TNV. The problem with this approach is that there are really six variables that need to be taken into consideration (the four parameters plus the TNV and the number of districts), and lacking a quantitative measure of the degree to which neutrality and a constant SR are approached, this program would be extremely complex and of rather limited value. Nevertheless, at a less formal level, it is clear that if one values neutrality, a wide RR, a relatively constant SR, and a high degree of competitiveness, it may be impossible to design a completely satisfactory districting plan.

Though a completely general approach does not seem useful at present, it is possible (by means of a few examples) to gain a more intuitive understanding of the incompatibilities among the parameter values and to understand the significance of these incompatibilities. Consider once again Figure 1. Figure 1 illustrates a districting plan that is neutral, has the maximum possible RR, and has a constant SR throughout that range. Relatively few seats, however, are competitive by commonly used standards (45–55 percent being the highly competitive range, 40–60 percent being somewhat competitive). Obviously more seats could be made competitive by simply creating a larger number of districts with DNV's close to 50 percent. If this were done "symmetrically," neutrality would be maintained. Moreover, if one district were left with a DNV = $(0 + \epsilon)\%$ and one with a DNV = $(100 - \epsilon)\%$, the RR would not be narrowed. It is readily apparent, however, that as more seats are made competitive, the SR will no longer be constant. At the extreme, the modified districting plan would still be neutral, have the maximum RR, and all but two of the districts would be extremely competitive. However, it would also have a SR of zero–i.e., be totally unresponsive to vote changes–throughout most of the range of votes.

In general, since DNV's must be spread out uniformly around the TNV to attain a constant SR, but bunched together in the competitive range to maximize competition, it can be seen that these two parameter values are basically incompatible (assuming an RR that is wide compared to the competitive range). This is clearest when TNV = 50%, as in Figure 1. However, the relevant considerations are the same, although the relationships among the specific values are more complex, when TNV ≠ 50%.

It is not merely the incompatibility between obtaining a constant SR and maximizing competitiveness that forms the root of the problem. If the TNV ≠ 50%, maintaining neutrality and maximizing competitiveness can also be mutually incompatible. Specifically, if the TNV is outside the competitive range, neutrality requires that at least half of the districts be safe since their DNV's must be at least as extreme as the TNV. In contrast, a non-neutral set of districts might include a larger number of districts that are competitive. As an example, consider a political unit in which the TNV = 60%, and assume for simplicity that there are only three districts, 1, 2, and 3. A neutral set of districts would require DNV's such as these:

	(a)	(b)	(c)	(d)	(e)	
1 ...	60	59 ...	55 ...	50 ...	45 ...	...
2 ...	60	60 ...	60 ...	60 ...	60 ...	...
3 ...	60	61 ...	65 ...	70 ...	75 ...	...

At most, only one district is highly competitive by the standards mentioned above. In contrast, a non-neutral set of districts could contain two highly competitive seats, along with one very safe seat, such as:

	(a)	(b)	(c)
1	50	50	52
2	50	55	53
3	80	75	75

Thus, in some circumstances, attempting to maximize the number of competitive districts may require violating neutrality, and conversely, retaining neutrality may require less than the maximum number of competitive districts.

The significance of this incompatibility among parameter values becomes clear if we try to establish standards for fair districting. While no consensus may exist on what the values should be for the four parameters, it seems likely that even if there were consensus on individual parameter values, compromise would still be necessary because of conflicts among the values chosen. It makes little sense, therefore, to establish goals for each parameter alone. Rather, fair districting plans can be arrived at, if at all, only by considering all four parameters simultaneously. This we do in the next section.

Standards for Fair Districting

Prescribing standards for fair districting is a risky business, especially in light of the incompatibilities we have shown to exist among the four responsiveness parameters. Moreover, it would be foolish to propose establishment of exact standards such as a RR of 27 to 78 percent, or a SR of 2.37. This means that in much of the discussion that follows we will speak of a "wide" RR, a "quite high" SR, and so on. Despite these difficulties, we think the model developed here provides a useful framework with which to begin a discussion of standards for fair districting.

First, however, a word about our approach. It would be a relatively simple matter to construct figures showing some of the possible trade-offs among parameter values. For example, assuming neutrality and a constant SR throughout the RR, it is a simple matter to show the nature of the trade-offs between the RR and the number of competitive districts. At some point this kind of general figure may be useful. Currently, however, we feel that the presentation of several specific examples provides a better way of discussing the question of fairness and how to achieve it.

Since the districting task is easiest when the total normal vote is exactly 50 percent and there is a large number of districts, we begin with this case.

TNV = 50%, Large Number of Districts. In this case we think that the best districting plan is one that is neutral, has a fairly wide RR, in which the SR is constant over significant portions of that range, and in which there is a reasonably large number of competitive districts by virtue of a higher swing ratio near the TNV than farther away from it. In support of such a prescription, we note first of all that there is no a priori reason to violate neutrality (such as to increase competitiveness as illustrated earlier). Second, a "fairly" wide RR will virtually assure a major party of some representation without creating extremely one-sided districts (see examples of this below). Third, a constant SR insures that vote changes will lead to seat changes in a relatively well-specified manner; having a large number of competitive districts means that the rate at which seats respond to votes is relatively high. And fourth, having an SR lower near the ends of the RR than in the middle makes it possible to have relatively constant swing ratios both in the middle and near the ends of that range (but a lower swing ratio at the ends than in the middle) without creating a large number of safe seats for each party.

In Figure 3 we illustrate several districting plans with these prescribed features. Any line starting at 20 or 30 percent of the vote and ending at 70 or 80 percent represents the SR for an alternative districting plan. Thus, for example, one could arrange the DNV's so that the SR = 1.0 from 20% to 45%, SR = 5.0 from 45% to 50%, SR = 2.5 from 50% to 60%, and SR = 1.25 from 60% to 80%. However, since neutrality requires a symmetric distribution of DNV's around 50 percent, we will assume that this is the case in the following discussion.

All of the districting plans presented provide a reasonable approximation to the standards noted above. Neutrality, as noted, is easily attained. The RR (either 20 to 80 percent, or 30 to 70 percent) seems quite acceptable, assuming the TNV really is a meaningful predictor of future votes. With the wider RR, the actual vote could deviate almost 30 percent from normal before one party would be completely denied representation. The SR is constant across ranges of at least 10 percent and there are no "flat spots" (in the RR) where the SR is zero. And, the number of competitive districts, especially in 3(a), is quite high.

Of course, even in this ideal case incompatibilities among parameter values are apparent. Awareness of these incompatibilities, however, aids us in prescribing appropriate

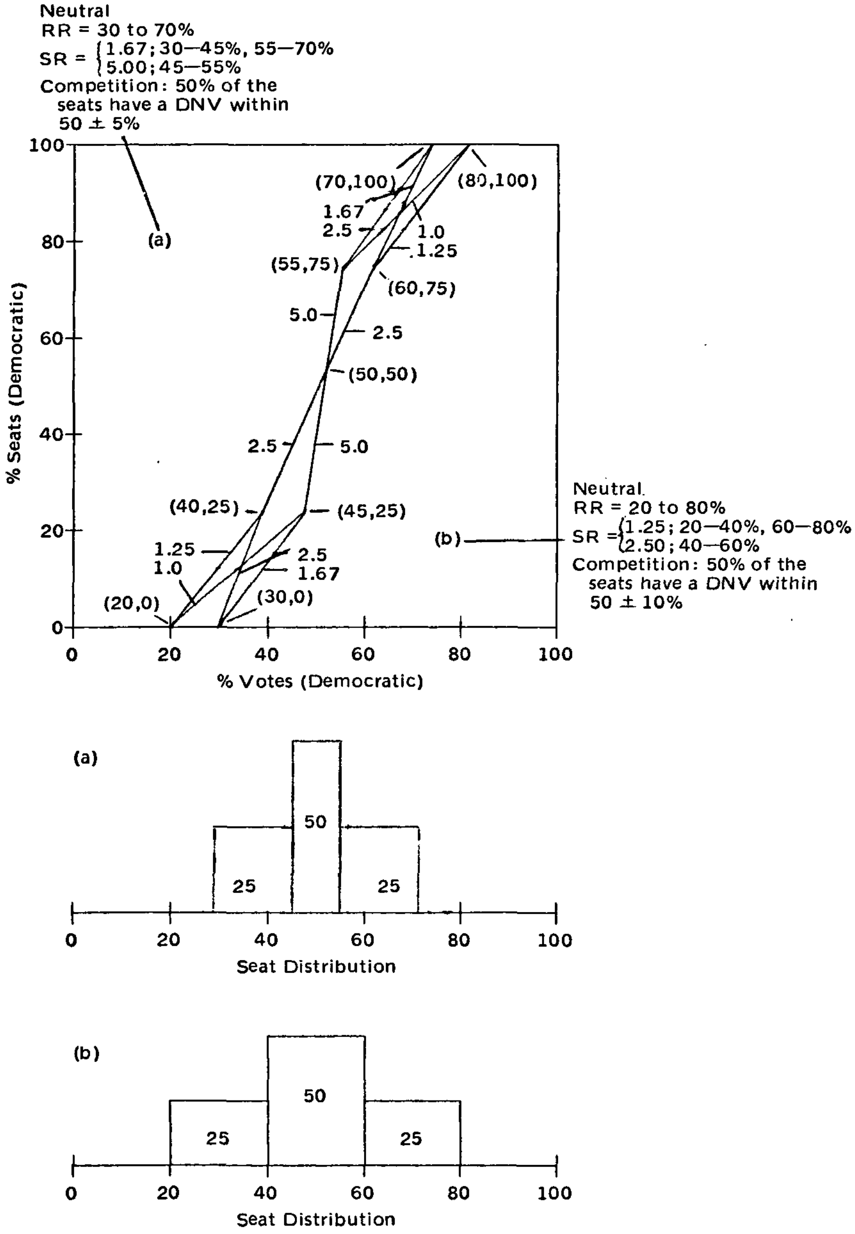

Figure 3. "Fair" Seats-Votes Curves for TNV = 50%

compromises to fit particular circumstances. Thus, if the TNV is regarded as a highly accurate predictor of what the vote in the constituency will be like over the life of the districting plan, then the narrower RR and the correspondingly higher SR's (as in Figure 3(a)) might be desirable. If the RR were fixed initially at 30 to 70 percent, however, we would still need to determine what the SR (or SR's) should be. To do this, consider what would happen if the actual vote dropped to 40 percent Democratic, only a 10 percent departure from the TNV. If the SR = 2.5 from 30% to 40%, the Democrats would win 25 percent of the seats, whereas an SR of 1.67 (from 30% to 45%) would give the Democrats only 17 percent of the seats. On these grounds the SR of 2.5 might be preferable. On the other hand, having the higher SR (2.5) near the ends of the RR necessarily lowers the number of competitive districts. Obviously, choosing between these (or other) SR's requires a subjective judgment; but at least the compromises involved are clear. Note that under these conditions, one might simply adopt a plan in which the SR is constant (2.5) throughout the entire RR.

Suppose, however, that it were considered unacceptable to have an electoral arrangement that would give no more than 25 percent of the seats to a party with 40 percent of the votes, and no seats at all to a party with up to 30 percent of the votes. These problems could be avoided by simply extending the RR. But if a constant SR were adopted, the number of competitive districts would be relatively low (approximately one-third), and the SR in the vicinity of the TNV (and throughout the RR) would be only 1.67. Here, then, it seems highly appropriate to extend the RR but to lower the SR near the ends of the RR.[12] The RR is thereby made sufficiently broad without unduly reducing the number of competitive districts.

It should be noted that an alternate but logically equivalent way of approaching the question of fairness is to ask whether the majority party has a great enough advantage rather than whether the minority party has a small enough disadvantage. Thus, for example, in reference to Figure 3 we previously asked whether it was fair for a party winning 40 percent of the votes to win only 25 percent, or perhaps only 16.7 percent, of the seats. Conversely, we could have asked: Is it fair for a party winning 60 percent of the votes to win "only" 75 percent or 83.3 percent of the seats? While this way of looking at things may suggest different political considerations,[13] we need not consider it further because of its mathematical equivalence to the approach above.

TNV ≠ 50%, Large Number of Districts. When the TNV differs from 50 percent, the mechanics of designing fair districting plans become somewhat more complicated. Moreover, when the TNV is far from 50 percent, the arguments in favor of neutrality may be much less powerful in light of the conflict between neutrality and competitiveness. Therefore it will be necessary to take another look at the question of neutrality. Again we think it most useful to proceed by the use of several examples.

First, suppose the TNV is close to 50 percent, say, 52 percent. In cases such as this the simplest approach is to construct the DNV's in a manner similar to that which was done for a TNV = 50%; that is, distribute the DNV's symmetrically about the TNV, bearing in mind the same considerations used earlier. Figure 4 shows one such plan. This figure is designed to be as similar as possible to the system in Figure 3(a). To accomplish this, the distribution of DNV's in Figure 4 is exactly comparable to that in Figure 3(a) except that it is centered at 52 percent rather than 50 percent.

Note that the SR's in Figure 4 are exactly comparable at every point to those in Figure 3(a). Despite the fact that the DNV's range from 32 to 72 percent, the RR is from 30 to 70 percent. The reason for this is easily seen. The most Democratic district has a DNV = $(72 - \epsilon)\%$. Therefore, when the TAV is $(22 - \epsilon)\%$ less than the TNV, this district and only this district will be won. (Technically the vote in this district will be exactly 50 percent, so one-half of the seat is won.) Since $(52 - (22 - \epsilon))\% = (30 + \epsilon)\%$, this is equivalent to saying that the

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[12] Such a compromise (not shown in Figure 3) might yield the following: SR = 1.0 from 20–30%, SR = 1.87 from 30–45%, SR = 2.4 from 45–55%, SR = 1.87 from 55–70%, SR = 1.0 from 70–80%. The RR = 20% to 80%, 42% of the seats have an expected vote of (50 ± 10)%, and the SR around the TNV is 2.4. A party winning 40 percent (30 percent) of the votes would win 29 percent (10 percent) of the seats.

[13] Most two-party systems in fact reward the winning party with more seats than votes (Rae, 1971, Ch. 4). This feature (though not necessarily the specific numbers used in our examples) can be defended on the grounds that it always yields a "working majority" even to a party winning a bare majority of the votes.

Democrats first begin to win seats when they win (30 + ϵ)% of the total vote. Similar reasoning shows that the least Democratic seat will be won when the TAV is 18 percent above the TNV, or 70 percent.[14] In terms of the SR, then, this districting plan is identical to that in Figure 3(a). The Democrats are closer to winning most or all of the seats, but that is owing to a TNV > 50%; the districting system itself is not biased against the Republicans.

Despite the similarities between the two figures, the districting plan in Figure 4 has fewer competitive districts than that in Figure 3(a). This characteristic occurs whenever a neutral districting plan is adopted and TNV $\neq$ 50%; that is, the number of competitive districts will be less than the number found in an otherwise identical districting plan for which the TNV = 50%.

Finally, observe that all considerations applying to Figure 3 also apply here as well. That is, the RR can be broadened or narrowed, the SR can be raised or lowered, and competition can be increased or decreased. But as before, the basic incompatibility of parameter values cannot be overcome.

A procedure such as the one outlined in Figure 4 could, of course, be used for any TNV. But it is obvious that as the TNV departs more and more from 50 percent, the number of competitive districts will decline to the vanishing point. Therefore, some other districting scheme must be developed for this situation. A clue as to what that scheme could be is contained in Figure 4. We noted that even though the TNV $\neq$ 50%, a symmetric distribution of DNV's *about* the TNV resulted in SR's centered *about* 50 percent (and, as we designed it, a relatively high value around the center and lower values away from 50 percent). But the actual vote is still expected to be near the TNV.

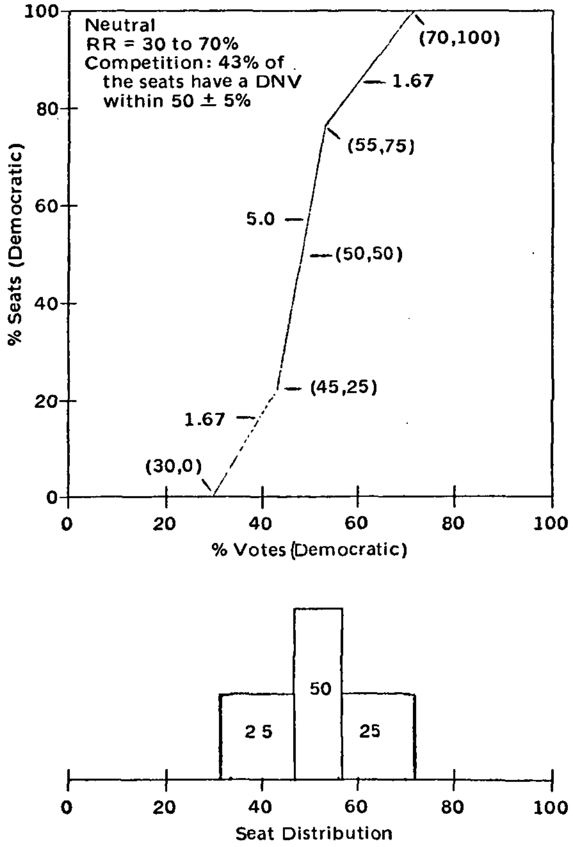

Figure 4. “Fair” Seats-Votes Curve for TNV = 52%

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[14] This is in accord with our formula for the RR described on p. 0000 above. Let $x = y = 20 - \epsilon$. Then the RR is from $(50 - (20 - \epsilon))\% = (30 + \epsilon)\%$ to $(50 + \epsilon + (20 - \epsilon))\% = 70\%$.

Hence, the SR is high in an area where votes are not expected (i.e., close to 50 percent) and, if the TNV is far from 50 percent, lower or even zero in the vicinity of the TNV. For example, a distribution of seats exactly like that in Figures 3(a) and 4, but centered around a TNV of 60 percent, would have only 16 percent of the seats in the highly competitive range; and, the SR would look exactly like those in Figures 3(a) and 4—i.e., it would be high around 50 percent but only 1.67 in the vicinity of the TNV.

Now, let us reverse the logic presented above. In order to increase the SR around the TNV, we need to distribute DNV's about 50 percent. The reason for this should now be apparent. As the actual vote fluctuates about the TNV, seats with DNV's near 50 percent are the ones that will move between the parties. The more seats there are with DNV's close to 50 percent, the more seats will change hands with a given vote change (i.e., the higher the SR will be around the TNV). However, it must also be remembered that the narrower the distribution of DNV's, the narrower the RR. Also, the more uniform the distribution of seats is around 50 percent, the more uniform the SR is (around the TNV).

If the TNV $\neq$ 50%, then, a fair districting plan requires construction of a set of districts centered at 50 percent. However, this prescription alone allows extremely wide latitude in the arrangement of districts, and some anomalous situations could arise. For example, ten districts with a TNV = 60% Democratic could be given DNV's of $(46 + \epsilon)\%$, $(47 + \epsilon)\%$, $(48 + \epsilon)\%$, $(49 + \epsilon)\%$, $(51 - \epsilon)\%$, $(52 - \epsilon)\%$, $(53 - \epsilon)\%$, $(54 - \epsilon)\%$, 100%, and 100% Democratic. But this would mean that (given our assumption of no constituency effects in the vote) if the TAV were 50 percent, or even as high as 56 percent Democratic, the Democrats would win only two seats! This kind of bizarre result can be prevented if it is required that when a party wins 50 percent of the votes it must win at least 50 percent of the seats. While conceivably there are circumstances in which this requirement might be relaxed (see n. 16 below), we think that it is generally appropriate since otherwise a party could win a majority of the votes but less than half of the seats. In imposing this condition, we are in effect requiring that the seats-votes curve pass through the (50,50) point, for if it did not, both parties would not meet the requirement. This in turn means that no more than half of the DNV's be greater than the TNV and no more than half be less than the TNV.[15]

At this point we still have a fair amount of latitude left in designing districting plans. In order to make some headway, let us proceed as follows. Let us first assume that proposed districting plans should be neutral; we will illustrate two districting plans under this assumption. Since it is our opinion that neutrality is generally a useful requirement, these plans illustrate our prescription for most cases in which TNV $\neq$ 50%. However, since our preference for neutrality is not an intense one, for reasons given below, we will shortly discard this requirement and illustrate a not-too-unreasonable, non-neutral districting plan.

Imposing neutrality means that the distribution of districts must be symmetric about the TNV. Let us assume that TNV > 50% Democratic and concern ourselves only with the districts for which DNV < TNV. Districts for which DNV > TNV can then be distributed in such a way that the entire distribution of DNV's is symmetric about the TNV. As noted, we will increase the size of the SR *around* the TNV the more we cluster these DNV's *about* 50 percent. However, we also noted that the RR and the uniformity of the SR were dependent on the manner in which these DNV's are distributed. What seems like a reasonable compromise is to distribute these seats symmetrically about 50 percent, and, as in previous cases, to distribute them so that the SR is higher near the TNV than farther away and so that the SR's are uniform over fairly wide ranges.

Two such districting plans are illustrated in Figure 5. Comparison and evaluation of the plans shown, along with other possible plans, should now be a familiar exercise. Figure 5(a), for example, features a larger number of competitive districts than 5(b) and therefore a higher SR around the TNV. On the other hand, under 5(a), the Democrats would win 95 percent of the seats with 65 percent of the

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[15] If the TAV = 50%, a party loses all seats for which

$$(1)\ \text{DNV} < \text{TNV},$$

and wins all seats for which

$$(2)\ \text{DNV} > \text{TNV}.$$

If more than half of the DNV's satisfy (1), then the party would win less than half of the seats. If more than half of the DNV's satisfied (2), it would win more than half of the seats; but then, of course, the other party would win less than half of the seats. Some seats may have DNV = TNV since they will be split evenly between the parties.

votes (only 5 percent above the TNV). Under 5(b) a 65 percent Democratic vote would yield a somewhat smaller Democratic majority (87.5 percent of the seats). Deciding which plan is preferable depends on how closely the actual vote is likely to be to the expected vote and on a judgment as to whether or not the Republicans should be given the chance (as in 5(a)) to win a lot of seats with the attendant risk of being virtually eliminated from the legislature.

Is the requirement of neutrality a good one? Consider the distribution of districts in Figure

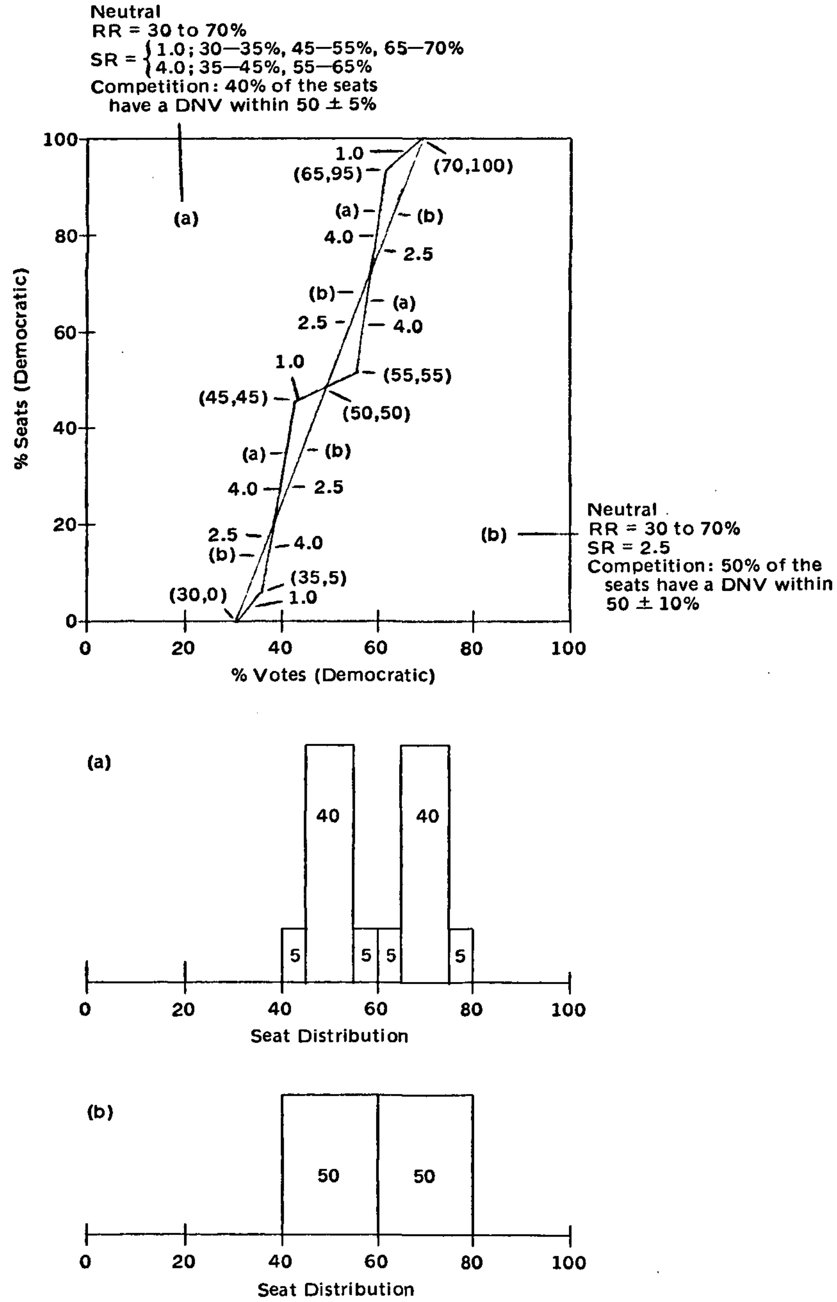

Figure 5. "Fair" Seats-Votes Curve for TNV = 60%

5(a). Half of the districts were distributed so that the mean DNV = 50%. Since TNV = 60%, the other half of the districts had to be distributed so that the mean DNV = 70%. Neutrality meant that they were in fact distributed symmetrically about 70 percent Democratic, with most of them being from 65 to 75 percent Democratic. If we violate neutrality, some of these districts could be made more competitive. The cost, however, would be that other districts would have to be made even less competitive. One such non-neutral districting plan is shown in Figure 6. The bias is readily apparent in the seats-votes curve. For example, if the Democrats won 55 percent of the votes, they would win 55 percent of the seats, whereas if the Republicans won 55 percent of the votes they would win 65 percent of the seats. We feel that the non-neutral plan gains very little at the expense of favoring one party. Admittedly, some seats are made more competitive. While they are still not very competitive in our example, they might be if the TNV were closer to 50 percent. But these competitive seats are gained at the expense of making some seats safer than they would otherwise be. For this reason, we do not feel the gain in competition is worth biasing the districting plan. Nonetheless, the gain in number of competitive seats is undeniable, and some may feel that this justifies the cost.[16] If so, Figure 6 serves as a model of how the districting plan can be

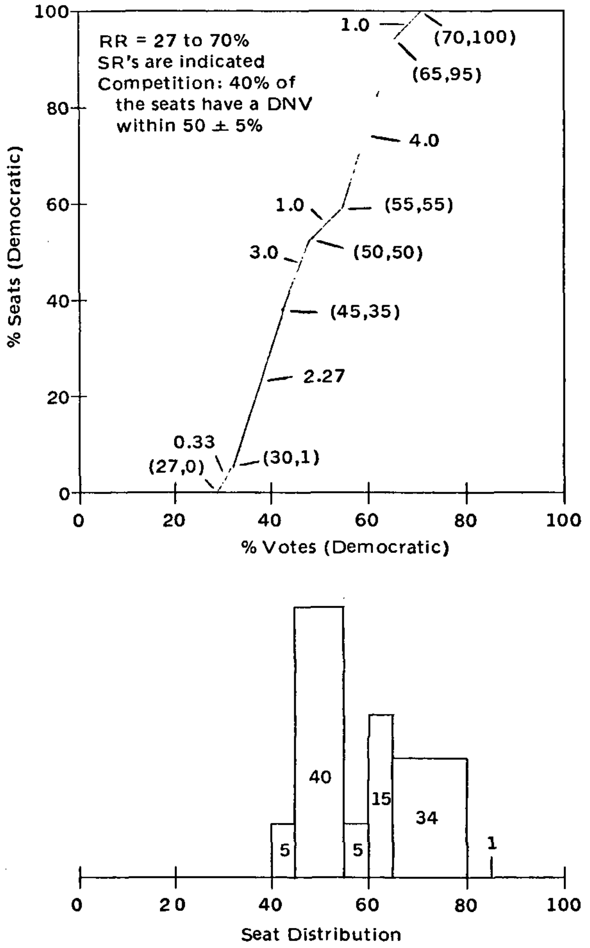

Figure 6. Non-Neutral Seats-Votes Curve for TNV = 60%

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[16] Some may find this reasoning more persuasive when the number of districts is very small. In the

implemented.[17] Note that there is no change between Figures 5(a) and 6 for all DNV's $<$ TNV.

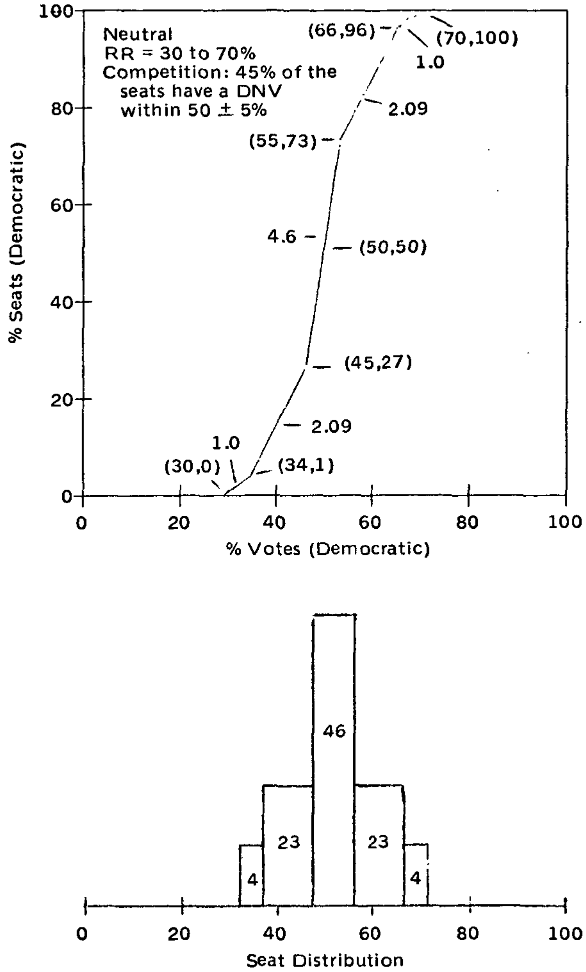

Figure 7. Alternative "Fair" Seats-Votes Curve for TNV = 52%

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examples on p. 1312, for example, there are only three districts. Consequently, neutral districting only allows one-third of the districts to be highly competitive, while a biased plan (biased even at the 50–50 point) raises that to two-thirds. The relative strength of the arguments may also depend on the structure of the political system. The argument in favor of neutrality is probably strongest when there is no intermediate structure such as states, some of which can be biased toward one party and some toward the other party. See our discussion of the "aggregation problem" in the conclusion.

[17] Other adjustments involving the neutrality question may have to be made if TNV $\gg$ 50%. First, it may be impossible to distribute half of the districts symmetrically about 50 percent. Second, while there exists a minimum percentage about which they can be distributed, a symmetric distribution about that percentage cannot be matched by the distribution of those districts for which DNV $>$ TNV. For example, let there be four districts and TNV = 80%. Two districts can be centered around 60 percent–e.g., DNV_1 = 55% and DNV_2 = 65%–if DNV_3 = DNV_4 = 100% for the other two districts. But the DNV's of these latter districts cannot be altered. Either the non-neutral plan just noted, or a neutral plan in which DNV = 60% for two districts and DNV = 100% for two districts, seems best. The problem in this case lies not in our prescription as such, but in the fact that the fair districting criteria simply cannot be met to the same degree when the TNV is so far from 50 percent.

We now have explicated two schemes for constructing districting plans when the TNV $\neq$ 50%. The first method, illustrated in Figure 4, is rather obviously defective when the TNV is far from 50 percent, and the second approach was devised to deal with this situation. But the second method can just as well be utilized when the TNV is close to 50 percent. If it is utilized, one new consideration arises. Namely, in distributing (up to) half of the DNV's about 50 percent, there is no requirement that for all of these districts DNV $<$ TNV (assuming TNV $>$ 50%). However, if they do not meet this condition, the distribution of these DNV's will overlap the distribution of DNV's distributed about the point (TNV + (TNV − 50))%.

A specific example illustrates this point and also shows how application of this method gives slightly different results from the first method when the TNV is close to 50 percent. Suppose TNV = 52%. A symmetric distribution of seats about 52 percent yields the seat distribution and the seats-votes curve shown in Figure 4. If we instead distribute half of the DNV's symmetrically about 50 percent, and half about 54 percent, we can derive the results shown in Figure 7. The seat distribution was derived by uniformly distributing half of the DNV's as follows: 12 over the range 32–45 percent, 26 over 45–55 percent, and 12 over 55–68 percent. To make the entire distribution symmetric, the other half of the DNV's was distributed as follows: 12 over the range 36–49 percent, 26 over 49–59 percent, and 12 over 59–72 percent. Thus the distributions of the two halves overlap considerably. At the same time, since they do not overlap completely, there are ranges at the extremes of the RR in which there are fewer districts than in the comparable portion of Figure 4; this accounts for the lower SR at the extremes of the RR. The SR in Figure 7 is complementary about 50 percent, while that of Figure 4 is not. For this reason, there are slightly more competitive districts in Figure 7. On the other hand, the DNV's are slightly more highly clustered about the TNV in Figure 4 (50 percent of DNV's within 5 percent of 52 percent) than in Figure 7 (46 percent of the DNV's within 5 percent of 52 percent). Therefore the SR in the middle of the RR is slightly lower in the latter figure.

Despite the small differences between them, both figures represent what we think are fair seats-votes curves for TNV = 52%. As should be the case, either districting method outlined above is fair when TNV is close to 50 percent. (In fact, they are equivalent at TNV = 50%.) However, as already shown, the more the TNV departs from 50 percent, the less satisfactory the symmetric distribution of DNV's about the TNV becomes, and the more appropriate the two symmetric distributions about 50 percent and (TNV + (TNV − 50))% become.[18]

Small Number of Districts. If the number of districts is small, as is true of congressional districts in many states,[19] approximating seats-votes curves such as those shown above may become difficult. In fact, with a very small number of districts, there will necessarily be large flat spots in the seats-votes curve. Figure 8 illustrates this by showing two curves for a plan involving five districts. If the DNV's are distributed as in 8(a), the solid line is the seats-votes curve. If the most extreme DNV's are moved to $(40 + \epsilon)\%$ and $(60 - \epsilon)\%$, as in 8(b), the seats-votes curve from 40–46 percent and 54–60 percent is represented by the dotted line. The former has the advantage that all districts are in what is usually considered the "highly competitive" range, but has the disadvantage of a RR from only 45 to 55 percent (strictly $(45 + \epsilon)\%$ to 55%). If the Democratic proportion of the vote dropped as much as 5 percent below normal, the Democrats would have no representatives despite obtaining up to 45 percent of the vote. The latter version avoids the narrow RR, but 2 of the 5 districts are at the edge of what is often considered the "somewhat competitive" range. And in either case there are some changes in votes (within the RR), up to 7 percent, which result in no changes in seats.

Thus, when the number of districts is small the seats-votes curve will necessarily depart somewhat from those shown previously. Yet the basic considerations remain the same: neutrality can be simply maintained, the RR can be made large enough to ensure that neither party's representation is entirely eliminated by minor vote swings around the TNV, districts can be clustered near 50 percent in order to achieve a high degree of competitiveness, and yet districts can be spread out sufficiently to attain a reasonably constant SR. Fair districting may be harder to achieve, but the considerations involved and the operational procedures for establishing particular parameter values remain the same.

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[18] Many of the schemes we have proposed as compromise solutions are reminiscent of piece-wise linear approximations to the nonlinear curve popularly referred to as the "Cube Law." This correspondence may be little more than coincidental since our formulations are theoretical prescriptions, while the Cube Law is proferred as an empirical description.

[19] Currently there are 35 states with fewer than 10 congressional districts.

Conclusion

We have argued that the formation of an equitable districting plan requires attention to a number of factors not ordinarily considered. Specifically, an evaluation of the fairness of a districting plan must include judgments about the presence or absence of neutrality, the range of responsiveness, the constancy of the swing ratio, and the amount of competition present. We have demonstrated that simultaneous attainment of certain combinations of values for these parameters is impossible; but we have also suggested compromises that we think are reasonable and result in districting plans with desirable properties.

We wish it were possible to conclude by proposing minimum standards which all districting plans should meet. Ultimately, we hope to do so. However, one problem keeps us from doing so at present. This is what we call "the aggregation problem." Assuming it is possible

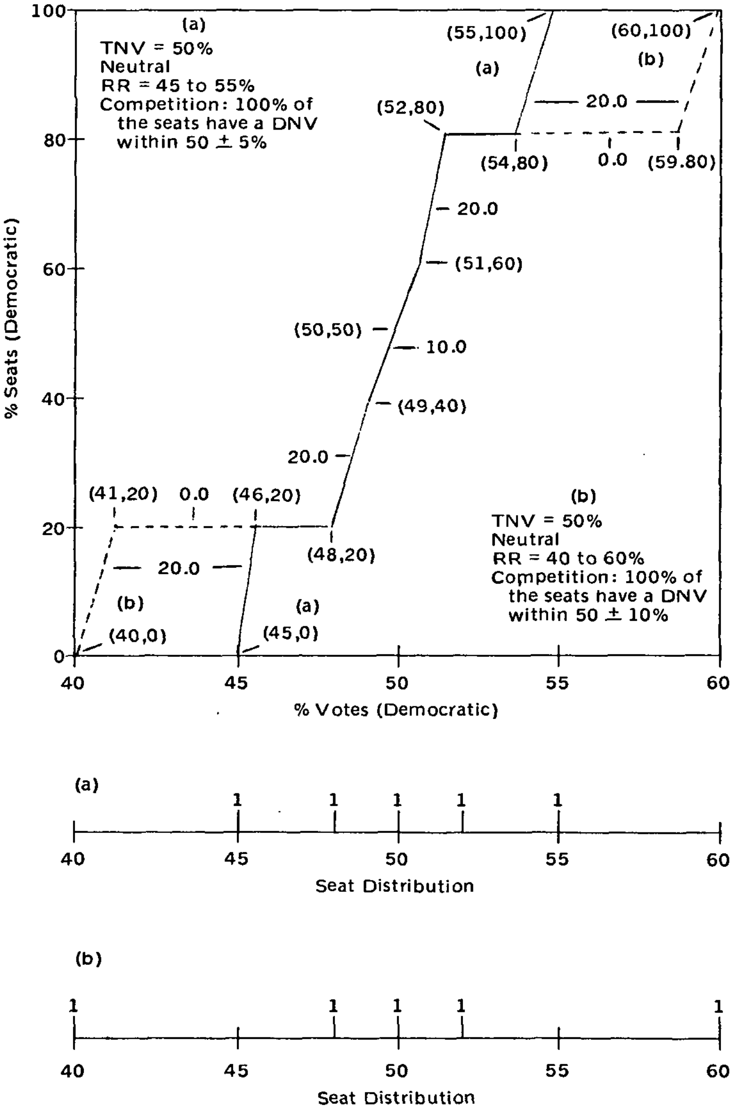

Figure 8. "Fair" Seats-Votes Curves for Five Districts

to arrive at agreed-upon standards for equitable districting plans within units of a federal system (e.g., American states), it is by no means obvious that these plans, when aggregated, will also result in an equitable arrangement at the aggregated level (e.g., at the national level). The presence of extremely one-sided districts, for example, may be viewed as inequitable and undesirable and can be easily avoided (except in extreme one-party states) when each state is considered in isolation. From a national perspective, however, such one-sided districts have historically meant that neither major party has been denied congressional representation, even when landslide elections have occurred. Thus, what might be thought of as inequitable at the state level, might also be viewed an extremely desirable feature from a national perspective.

Despite this remaining difficulty, as well as the possibility that additional complications can be brought into the model, we believe that the districting schemes derived here can serve as standards both for comparison and evaluation of existing districting plans and for the construction of equitable districting plans in the future.

Appendix

The relationship between the distribution of DNV's and the seats-votes curve is best shown by the kind of illustration in the figures. However, unless care is taken in determining precise distributions, minor problems can arise (such as a party winning all of the votes but winning slightly less than all of the seats). The use of ϵ values avoids these problems. Also, in many figures no district is created which has a DNV = TNV. We will speak of a uniform distribution even though this feature causes slight departures from uniformity.

For those who are interested, the exact distributions of DNV's for all figures are given below. All DNV's are given in percent Democratic.

Figure 1: 1 district each with a DNV of $0 + \epsilon, 1 + \epsilon, \ldots, 49 + \epsilon, 51 - \epsilon, 52 - \epsilon, \ldots, 100 - \epsilon$.

Figure 2: (a) 5 districts each with DNV's of $40 + \epsilon, 41 + \epsilon, \ldots, 49 + \epsilon, 51 - \epsilon, 52 - \epsilon, \ldots, 60 - \epsilon$; (b) 2 districts each with DNV's of $25 + \epsilon, 26 + \epsilon, \ldots, 49 + \epsilon, 51 - \epsilon, 52 - \epsilon, \ldots, 75 - \epsilon$.

Figure 3: (a) 1 district each with DNV's of $30.0 + \epsilon, 30.6 + \epsilon, \ldots, 44.4 + \epsilon, 55.6 - \epsilon, 56.2 - \epsilon, \ldots, 70.0 - \epsilon$; 5 districts each with DNV's of $45 + \epsilon, 46 + \epsilon, \ldots, 49 + \epsilon, 51 - \epsilon, 52 - \epsilon, \ldots, 55 - \epsilon$; (b) 1 district each with DNV's of $20.0 + \epsilon, 20.8 + \epsilon, \ldots, 39.2 + \epsilon, 60.8 - \epsilon, 61.6 - \epsilon, \ldots, 80.0 - \epsilon$; 1 district each with DNV's of $40.0 + \epsilon, 40.4 + \epsilon, \ldots, 49.6 + \epsilon, 50.4 - \epsilon, 50.8 - \epsilon, \ldots, 60.0 - \epsilon$.

Figure 4: 1 district each with DNV's of $32.0 + \epsilon, 32.6 + \epsilon, \ldots, 46.4 + \epsilon, 57.6 - \epsilon, 58.2 - \epsilon, \ldots, 72 - \epsilon$; 5 districts each with DNV's of $47 + \epsilon, 48 + \epsilon, \ldots, 51 + \epsilon, 53 - \epsilon, 54 - \epsilon, \ldots, 57 - \epsilon$.

Figure 5: (a) 1 district each with DNV's of $40 + \epsilon, 41 + \epsilon, \ldots, 44 + \epsilon, 56 - \epsilon, 57 - \epsilon, \ldots, 60 - \epsilon, 60 + \epsilon, 61 + \epsilon, \ldots, 64 + \epsilon, 76 - \epsilon, 77 - \epsilon, \ldots, 80 - \epsilon$; 4 districts each with DNV's of $45 + \epsilon, 46 + \epsilon, \ldots, 49 + \epsilon, 51 - \epsilon, 52 - \epsilon, \ldots, 55 - \epsilon, 65 + \epsilon, 66 + \epsilon, \ldots, 69 + \epsilon, 71 - \epsilon, 72 - \epsilon, \ldots, 75 - \epsilon$; (b) 1 district each with DNV's of $40.0 + \epsilon, 40.4 + \epsilon, \ldots, 49.6 + \epsilon, 50.4 - \epsilon, 50.8 - \epsilon, \ldots, 60 - \epsilon, 60.0 + \epsilon, 60.4 + \epsilon, \ldots, 69.6 + \epsilon, 70.4 - \epsilon, 70.8 - \epsilon, \ldots, 80 - \epsilon$.

Figure 6: 1 district each with DNV's of $40 + \epsilon, 41 + \epsilon, \ldots, 44 + \epsilon, 56 - \epsilon, 57 - \epsilon, \ldots, 60 - \epsilon, 65.45 - \epsilon, 65.90 - \epsilon, \ldots, 79.85 - \epsilon, 80 - \epsilon, 83 - \epsilon$; 4 districts each with DNV's of $45 + \epsilon, 46 + \epsilon, \ldots, 49 + \epsilon, 51 - \epsilon, 52 - \epsilon, \ldots, 55 - \epsilon$; 3 districts each with DNV's of $61 - \epsilon, 62 - \epsilon, \ldots, 65 - \epsilon$.

Figure 7: 1 district each with DNV's of $32.0 + \epsilon, 33.1 + \epsilon, \ldots, 44.1 + \epsilon, 55.9 - \epsilon, 57.0 - \epsilon, \ldots, 68.0 - \epsilon, 45.0 + \epsilon, 45.4 + \epsilon, \ldots, 49.8 + \epsilon, 50.2 - \epsilon, 50.6 - \epsilon, \ldots, 55.0 - \epsilon, 36 + \epsilon, 37.1 + \epsilon, \ldots, 48.1 + \epsilon, 59.9 - \epsilon, 61.0 - \epsilon, \ldots, 72.0 - \epsilon, 49 + \epsilon, 49.4 + \epsilon, \ldots, 53.8 + \epsilon, 54.2 - \epsilon, 54.6 - \epsilon, \ldots, 59.0 - \epsilon$.

Figure 8: (a) 1 district each with DNV's of $45 + \epsilon, 48 + \epsilon, 50, 52 - \epsilon, 55 - \epsilon$; (b) 1 district each with DNV's of $40 + \epsilon, 48 + \epsilon, 50, 52 - \epsilon, 60 - \epsilon$.

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The Conditions of Racial Violence in American Cities: A Developmental Synthesis*

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This article analyzes the social and political conditions associated with the incidence of racial violence in a sample of 119 American cities. Data on the incidence of racial disorders are drawn from newspaper accounts compiled by the Lemberg Center for the Study of Violence during the period 1967–1969. A total of 334 disorders are analyzed.

Two alternative hypotheses are examined. The first assumes that the causes of the black urban riots are rooted largely in the disorganized environment of socially marginal individuals. The second attempts to locate the outbreak of rioting primarily within a closed and unresponsive political system. Paradoxically, the results tend to provide empirical support for both theoretical perspectives. At the same time, the data suggest the need to reformulate and revise conventional interpretations of the black urban riots. This is done by synthesizing and testing a developmental model which implies a curvilinear relationship between the incidence of racial violence on the one hand, and black political development on the other.

Despite a burgeoning literature, a behavioral enigma still shrouds the black urban riots of the latter 1960s. Why, for example, was the incidence of rioting apparently most severe where black social conditions had shown the greatest signs of improvement (Ford and Moore, 1970; Spilerman, 1970)? And why did rioting break out on a mass scale when the local political environment itself appeared to hold out the promise of even greater racial progress following earlier civil rights triumphs (Rainwater and Yancey, 1967, p. 193)? While nearly a decade has elapsed since the peak of racial unrest in the nation's cities, no empirical study has come forward which convincingly explains these paradoxes.

Instead, several recent studies purport to show that most socioeconomic and political conditions of the central city have little to do with the reported incidence of racial strife, once controls are introduced for the nonwhite population size and region (Spilerman, 1970; Jiobu, 1971; Morgan and Clark, 1973). The prevailing interpretation of this result suggests that ghetto violence can best be viewed as a collective response to national forces which cut across political jurisdictions and supersede local black grievances. These allegedly include, but are not limited to, rising black expectations, broken presidential promises, vacillating federal action and the suggestive impact of the national news networks, particularly television (Spilerman, 1970, p. 645).

This analysis will present new evidence which challenges this interpretation. In particular, it will be shown that local conditions revolving around the economic well-being of blacks, social marginality and institutional closure exerted substantial effects on the propensity of urban blacks to riot independently of the nonwhite population size. I will reinterpret these conditions within a developmental context. In addition, I will present data which demonstrate why a separate statistical control for region may tend to obscure the effects of these conditions. My immediate objective is to develop an empirical theory of racial violence which is consistent with reported data. My ultimate goal is to contribute to the continuing quest for a behavioral theory of social and political violence.

The results and discussion will be presented in three main sections. The first develops a theoretical model to explain the outbreak of black urban riots in terms of two contending perspectives—social marginality and institutional closure. The second section tests the adequacy of the model by correlation and regression analysis. In this section, data are presented which document the independent effects of institutional closure over and above those explained by economic and social conditions. The final section is a theoretical exploration and behavioral synthesis of the results. Here, I formulate and test a developmental model which implies a curvilinear relationship between the incidence of black urban riots and black political development.

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Methodology

Data on racial violence and community characteristics were gathered for a simple random sample of 119 American cities for 1967–1969. The sample of cities was drawn from a universe of 207 SMSA cities as of the 1960 census. Most cities are greater than 50,000 population and all have 1,000 or more black households. Most can be classified as meeting "critical mass" standards of population size.[1]

Racial violence data were obtained from the Lemberg Center for the Study of Violence. The Lemberg (1968) data consist of coded and uncoded newspaper reports that were collected, synthesized and collated by the Center in tabular and scenario form.[2] I gathered data on the socioeconomic and institutional characteristics of central cities from a variety of sources, including *The 1960 Census of the Population–Selected States* (1961), *The 1967 City and County Data Book* (1967), *The Municipal Yearbook* (1967, 1970), *The 1967 Elementary and Secondary Education Directory* (1969), *Uniform Crime Reports, 1966* (1967) and Taeuber and Taeuber's *Negroes in Cities* (1965).

Using the Lemberg Center's definition (1968, p. 2), I have defined a racial disorder as a "race-related incident involving crowd behavior, characterized by either damage to persons or property and/or defiance of civil authority." Crowd behavior refers to the activities of four or more people acting in concert. Defiance of civil authority includes verbal derision (i.e., chanting, cursing or taunting government officials), disobedience of civil authority, and physical attacks upon such authorities or property. Though the participants are usually black, they may be black or white, while the targets selected for attack may be persons or property.[3] Using this definition, I identified a total of 334 community disorders for the sample of 119 cities from 1967–1969.

Disorder Propensity. Past studies of comparative urban strife have tended to employ the absolute frequency of disorders as their primary measure of disorder propensity (Downes, 1968; Spilerman, 1970; Morgan and Clark, 1973).[4] Regrettably, there appear to be two methodological problems with this measure as a dependent variable. First, the absolute frequency distribution is highly skewed and peaked. Although 4 of 119 cities in the sample experienced more than ten racial disorders from 1967–1969, most witnessed far fewer. Twenty-six cities, in fact, experienced no disorders at all, while the bulk (approximately 54 percent) experienced from one to three disorders. Second, the absolute frequency is highly correlated with the logarithms of the total (.60) and nonwhite (.57) population sizes. Generally, larger cities experience more disorders because they have more potential riot participants and thus more numerous frictional encounters between blacks and whites, the police, and other city officials.

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[1] Joe Feagin and Harlan Hahn (1973, p. 122) have aptly defined the "critical mass" as the "minimum ghetto size necessary for generating and sustaining a riot."

[2] The chief problems in using the Lemberg data center on the inherent biases of news reporting and the reluctance of local officials to admit the outbreak of racial strife. Newspapers are notoriously fickle about what is or is not printed. Politically, it is unpopular and embarrassing for local officials and the press to admit that a riot occurred in their city. Therefore, relying solely on journalistic accounts and statements by city officials may at times be misleading, and verification may not always be possible because of a lack of official cooperation. While the Lemberg collection system was by no means foolproof or infallible, in that trained clippers can err and newspapers can fail to report racial disorders consistently, the Lemberg data appear to represent the most accurate data base available on the black urban riots of the late 1960s (Feagin and Hahn, 1973, pp. 101–08). I am indebted to Ms. Terry Ann Knopf for supplying these data.

[3] In principle, there is no definitional bias toward the reporting of incidents which primarily involve blacks or personal attacks. However, the definition is subject to the criticism that "race-relatedness" is not defined. Hence, the reporting of "race-related" violence may vary over time because of changing definitional standards. This definition may also set an artificially low limit on the number of participants which are necessary for a disorder to be counted. This latter bias toward over-reporting may, on the other hand, compensate somewhat for the under-reporting of riots by the press.

[4] Although this measure tends to treat each incident of violence as if it were equal to every other incident, it does not seem unduly restrictive given that: (1) reported incidents of community violence are far more accurate and reliable than indicators of their scope and severity, such as length, number of participants, human casualties and estimated property damage; (2) these latter intensity measures are more likely to be influenced by the particular nature and character of the police response in each city; and (3) the absolute frequency and four selected intensity measures–length, number of participants, casualties and property damage–can be reduced for all practical purposes to a single principal component (65 percent explained variance) which loads uniformly high on each. Thus, little information appears to be gained by inclusion of additional intensity measures in the analysis.

To define a measure of disorder propensity which is approximately normally distributed, I applied a logarithmic transformation of the form $\log(1 + F_i)$ to the data, where F_i is the total number of disorders which occurred in each city from 1967–1969. Since 26 cities in the sample did not report any community disorders, the unitary constant was added to establish a zero base point and to preclude negative logarithmic values. To adjust for the confounding effects of population size, residual differences, $\Delta\log(1 + F_i)$, were computed for each city in the sample by taking the difference between this log transformed measure, $\log(1 + F_i)$, and its regression estimate, $\widehat{\log(1 + F_i)}$, on the logarithm of the nonwhite population size, P_i.[5] Quantitatively:

$$\Delta\log(1 + F_i) = \log(1 + F_i) - \widehat{\log(1 + F_i)}$$

where

$$\widehat{\log(1 + F_i)} = a + b \log P_i.$$

1. A Theoretical Model of Disorder Propensity

A systematic review of the literature on the black urban riots suggests that many extant interpretations are based on two broad, contending perspectives: (1) social marginality and (2) institutional closure (Feagin and Hahn, 1973). The first tends to view riot participants as social deviants and rioting as an act of collective criminality. The second, by contrast, tends to see rioters as political dissidents and riots as acts of political rebellion.

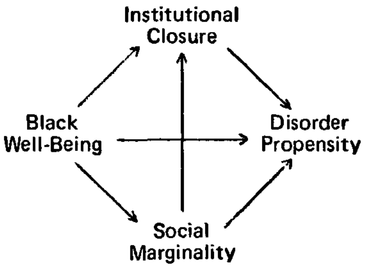

Figure 1. A Theoretical Model of Disorder Propensity

The hypothesized joint effects of social marginality and institutional closure on the disorder propensity are diagrammed in Figure 1. Since class variables are generally uniform predictors of community stability and political influence, each factor is assumed to vary as a function of black well-being. The direct path from social marginality to institutional closure is premised on the assumption that conditions of social disorganization tend to further retard the political participation of urban blacks and hence reduce their access to municipal institutions. The purpose of this section is to develop the key assumptions undergirding marginality and closure theory.

Social Marginality. The theory of social marginality seems critical to a number of popular, official and academic interpretations (Feagin and Hahn, 1973). These range from the "wild youngsters" and "riff-raff" explanations to rather sophisticated theories of collective violence, such as structural strain, alienation, frustration-aggression, relative and absolute deprivation, and the revolution of rising expectations. While the former tend to emphasize riot participation by the more youthful, criminal elements of society, the latter are generally imbedded in social-psychological perspectives and look more to racial injustices and the alleged grievances of riot participants.

Notwithstanding these differences, each theory tends to assume that riot participants are socially marginal individuals who vent their frustrations, perceived deprivations and thwarted aspirations in aggressive riot behavior (Feagin and Hahn, 1973, pp. 6–24). Violence thus becomes an acceptable recourse for marginal individuals because of their ostracism from the economic and social mainstreams of urban society. It is seen as a feasible tactic because of their relative immunity from conventional norms and sanctions.

The social roots of marginal (read deviant) behavior are generally presumed to reside in the social instability and pathological conditions which have emerged in black ghettos because of family breakdown, juvenile delinquency and crime. These conditions have been interpreted as stemming ultimately from the long-term disorganizing effects of American slavery, racial discrimination and black migration. More immediately, marginality theory has tended to emphasize the disorganizing effects of high unemployment and low family income (Moynihan, 1965; Goldman, 1969).[6]

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[5] An analysis of the residual differences reveals conformity to the usual assumptions of regression analysis, namely, independence of error terms, zero mean, constant variance and normality (Draper and Smith, 1966, pp. 86–103).

Institutional Closure. In contrast to marginality theory, the perspective of institutional closure is founded on a political, as opposed to a social, set of assumptions (Silver, 1968; Skolnik, 1969; Nieburg, 1969; Fogelson, 1971; Feagin and Hahn, 1973). Hence, closure theory attributes most urban rioting to the institutional injustices of a flawed society rather than the flawed character of marginal individuals. Given this perspective, the act of rioting is seen to constitute a primitive demand by powerless blacks for greater material rewards and for greater control over the political institutions which govern and regulate their lives. This resort to violence is allegedly justified first, by their social and economic deprivation; second, by their residential segregation in ghettos; third, by their systematic exclusion from the primary labor market; and lastly, by their political subjugation to an institutionalized neocolonialism (Long, 1971).

Finally, a perspective of institutional closure tends to interpret recent urban rioting in the context of the continuing black struggle for civil rights and racial parity. In this context, riots are viewed as but one more act in a political drama between intransigent powerholding groups and powerless blacks. Though admittedly diffuse and understandably disorganized, riots are alleged to represent inchoate attempts by ghetto blacks to achieve social and political objectives they had not been able to achieve through nonviolent protest and the conventional channels of pluralist politics. Ghetto rioting, in effect, becomes the pursuit of politics by violent means.[7]

2. Results

History reveals that the absolutely downtrodden seldom revolt or resort to violence (Davies, 1969; Huntington, 1968). Though scholarly interpretations tend to vary, there appears to be a general consensus on the underlying social logic of political rebellion. Based on a "needs" logic, "slaves" rarely rebel because they are too preoccupied with satisfying basic human needs (Maslow, 1954; Bay, 1965). In general, their immediate concerns about physical survival and security are deemed to far outweigh their sense of personal oppression, loss of self-esteem or recognition of thwarted opportunities.

Following history and this logic, it is not surprising that urban racial violence has been found to be generally greater where the quality of life for blacks is less oppressive (Ford and Moore, 1970; Spilerman, 1970). Table 1 presents the results of this study for eight selected indicators of black well-being. These are grouped along two conceptual dimensions: (1)

Table 1. Correlations of Disorder Propensity and Selected Indicators of Black Well-Being

	Disorder Propensity
Absolute Well-Being	
Percent NW high school education	.05
Percent NW males crafts-foremen	.05
Percent NW families poor[a]	–.31***
Percent NW housing costs[b]	–.26**
Relative Well-Being	
Educational inequality[c]	–.21*
Occupational inequality[c]	–.27**
Personal income inequality[c]	–.39***
Family income inequality[c]	–.35***

[a] Families making less than $3,000 annual income.

[b] *Ratio of median housing rental to median non*white family income.

[c] Gini index.

*p $<$.05
**p $<$.01
***p $<$.001

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[6] In the empirical riot literature, the presumed bases for a social marginality interpretation tend to focus on the predominantly economic patterns of vandalism observed in most disorders; the youth, low-class background and acquisitive motivations of many self-reported rioters; and their past criminal records (Rossi and Berk, 1970; Berk and Aldrich, 1972; Caplan and Paige, 1968; Fogelson and Hill, 1968).

[7] The presumed empirical bases for a politics of violence perspective tend to center on: (1) the results of surveys which indicate strong criticism and dissatisfaction among blacks, particularly young militants, regarding existing economic arrangements and local governmental institutions (Caplan and Paige, 1968; Campbell and Schuman, 1968; Tomlinson, 1969; Caplan, 1970); (2) the symbolic nature of many riot targets, particularly the police and police property, as well as the disruptive and rebellious types of riot behavior reported in the press; and (3) the events and circumstances which precede and follow the outbreak of racial violence, especially the prior frictional interaction between police and ghetto residents, the escalation of violence following protest rallies and demonstrations over the actions of local authorities, and the overtly political reactions of local authorities to the riots themselves (Feagin and Hahn, 1973, pp. 31–54).

the respective class position of urban blacks and (2) their social status relative to whites.

The results, as expected, provide additional confirmation of the well-being hypothesis, namely, that racial strife is more likely to occur in cities where economic conditions are better for blacks, both absolutely and relatively, when compared with whites. But the conditions which appear to be most instrumental are those which revolve primarily around the financial situation of black families. Cities with high proportions of black families above the poverty threshold are significantly more likely to experience racial violence than cities where large segments of the black community still live below the poverty line.[8]

Social Marginality. This study analyzes two dimensions of social marginality. These are the extent of breakdown in the black family structure and the degree of social disorganization in the black community. Table 2 presents the correlations between the disorder propensity and 14 selected indicators. The results show that a number of marginality indicators are significantly related to the incidence of racial strife in the predicted direction, even when controlled for a key indicator of black well-being—the proportion of nonwhite families who are poor (see Figure 1). Thus, racial violence is more likely to occur in cities where nonwhite family structures are disintegrating, as reflected in higher divorce and separation rates and higher illegitimacy rates, than where such rates are lower. Other conditions of social disorganization which appear to increase a city's disorder-proneness are demographic dislocations and changes in the size of the nonwhite population, nonwhite residential mobility, nonwhite male transiency and the overall social control problem. The exacerbating effects of the latter two conditions appear to be borne out by the results for the nonwhite male to female age inequality index and the police density.

The age inequality index[9] was specifically

Table 2. Correlations of Disorder Propensity and Selected Indicators of Social Marginality

	Disorder Propensity	
	r_0	r_1
Family Breakdown		
Percent NW women divorced or separated	.25**	.28***
Percent NW one-parent homes	.01	.29***
Percent NW children in one-parent homes	–.04	.10
NW birth rate[a]	.19*	.10
NW fertility ratio[a]	.15	.08
NW illegitimacy rate[b]	.37**	.47***
NW male-female age inequality[c]	.20*	.06
NW male-female job inequality[c]	–.11	–.05
Community Disorganization		
Percent NW population change	.16*	.12
Percent NW change in residence	.24**	–.11
NW high school dropout rate	.00	–.06
Robbery density	.13	.07
Auto theft density	.09	.01
Police density	.23**	.20*

r_0 Simple Pearson product-moment correlation.
r_1 Partialled for the proportion of nonwhite families who are poor.
[a]Data for 31 cities were not available.
[b]Data for 59 cities were not available.
[c]Gini index.
[d]Percent of all nonwhites, age 14 to 17, who are not enrolled in high school.
$^{*}p < .05$
$^{**}p < .01$
$^{***}p < .001$

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[8] The poverty threshold used in this analysis is a national average which is based on the 1960 census definition of subsistence poverty. Hence, it is not wholly satisfactory for cross-urban analysis. A more rigorous operationalization would adjust this measure for variations in the consumer price index across the sample of cities and over the period, 1960–1967. In the absence of these data, however, I decided to proceed with the 1960 figure.

[9] This measure is a Gini index and takes on values

included to measure the reported under-enumeration of black males in the 1960 census (Bergman and Gray, 1974). A value of zero indicates total residential stability in the black community. Equal proportions of black males and females are to be found in each age-specific category. Values greater than zero indicate that the reported proportion of black males was less than the reported proportion of black females in selected age categories. Discounting the slightly greater mortality of males in higher age categories, this indicator appears to reflect the greater residential transiency of ghetto youths and young adults, two age groups which contributed perhaps the bulk of riot participants in most cities (Kerner, 1968, pp. 128–29; Caplan and Paige, 1968). The positive correlation of this measure with the disorder propensity thus implies that conditions of social disorganization among black males measurably increased the likelihood of racial violence.

The police density was included as an indirect measure of the "order maintenance" role of the police, since studies of police behavior suggest that the bulk of all police work in large cities is devoted to this activity (Wilson, 1968). The positive correlation between the disorder propensity and the police density may indicate that racial strife is more likely to occur in cities with documented histories of community discord.[10] While this result may also be construed as supporting a criminality thesis of rioting, the null results for the robbery and auto theft densities do not appear to corroborate this latter interpretation.

Institutional Closure. Following Eisinger's (1973) study of urban protests, I conceptualized institutional closure in terms of a city's political opportunity structure. Here, I assumed that black riot participation is conditioned and shaped by the extent to which (1) the formal political structure is open to black political participation, (2) government agencies and institutions are responsive to black needs and grievances, and (3) blacks are formally represented in the social and institutional life of the central city.

The objective of this section is to test whether closed and unresponsive political systems are more or less conducive to the outbreak of black rioting. Two hypotheses are considered—one based on simple deprivation assumptions, the second on the more complex assumptions of cultural contact-conflict (Armor, 1972, 1973; Pettigrew et al., 1973). The deprivation hypothesis assumes that violence will be greatest in those cities where the institutional structure is manifestly the most closed. The cultural contact-conflict hypothesis, by contrast, allows for a combination of results. That is, violence may be higher or lower depending on the economic and social condition of blacks, the perceived racial salience of an issue, and the opportunities for aggressive behavior.

The results are reported and discussed for each level of institutional closure. To assess the independent effects of selected indicators, I have controlled these results for reported levels of black well-being and social marginality, as reflected, respectively, in the proportion of nonwhite families with incomes below the poverty level and the proportion of nonwhite women who are divorced or separated (see Figure 1).

The first set of results in Table 3 reports the relationship between reform governmental structures and the propensity of urban disorders. A burgeoning school of revisionist scholars has contended that reform structures—manager-council government, at-large aldermanic districts, and non-partisan elections—are less responsive and accountable to racial minorities than nonreform structures (Lee, 1960; Banfield and Wilson, 1963; Hays, 1964; Lineberry and Fowler, 1967; Hawley, 1973). Thus, an appointed manager who serves at the pleasure of the city council and performs for the praise of his professional peers is deemed less responsive and accountable than a directly elected mayor whose margin of victory may depend quite crucially on the support of minority voters. In like manner, at-large districts are considered to offer residentially concentrated minorities few-

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between 0 and 1. Gini indices were calculated by numerical quadrature integration from the formula:

$$\text{Gini index} = \frac{\sum_{i=1}^{N} [X_i - f(X_i)]\,[X_i - X_{i-1}]}{.5}$$

where X_i is the cumulative distribution of one variable, $f(X_i)$ is the cumulative distribution of the second, and N is the total number of cases (Russett, 1965, pp. 117–21).

[10] An alternative explanation consistent with this finding is that police surveillance and harassment precipitated many disorders (Fogelson, 1971; Nieburg, 1969). However, this interpretation is not fully supported by the data since higher police densities appear to be primarily reflective of higher levels of reported property crime, rather than an excess of law enforcement zeal. For example, the observed correlations between the police density and the robbery and auto theft densities were respectively .47 and .44.

er opportunities for representation and political access to council members and candidates than ward districts (Lineberry and Fowler, 1967, p. 715). Finally, the institution of nonpartisan elections is seen to handicap minority group candidates by undermining the grassroots base of political parties, reducing the turnout of low-income and poorly educated voters who rely on partisan voting cues, and encouraging a politics of personality, style and candidate image over one of issues (Lee, 1960, pp. 49–69).

Some research suggests that the closure effects of reform institutions may be cumulative in nature. The more a city is dominated by reform institutions, the less responsive its officials are to minority needs and grievances. Thus, Lineberry and Fowler (1967, p. 716) have used a linear model to explain the lower expenditure and taxation patterns of reform governments and their allegedly reduced responsiveness to the social needs and political cleavages which divide the central city. Eisinger (1973, p. 23) on the other hand, has recently advanced a curvilinear model to explain the effects of reform structures on urban protests. His data tend to support the contention that protest is most likely to occur where formal political structures reflect a mixture of open and closed characteristics.

The data reported here, however, suggest that racial disorders do not appear to be a response by urban blacks to closed formal governmental arrangements, either in their individual or cumulative forms. Although disorders are more likely to occur in cities having unreformed political structures, particularly those with mayor-council forms of government, the observed relationships do not seem to reflect the impact of formal structure so much as they do population characteristics. That is, cities with unreformed structures may appear more disorder-prone because they have higher proportions of black families with incomes above the poverty level and greater social disorganization in the black community.

Formal political structures, of course, provide only crude indications of the political and policy biases which may work to the disadvantage of racial minorities. More direct measures, perhaps, are to be found in the institutional processes and policy outputs which are specifically intended to deal with the grievances and needs of minority groups. To measure the responsiveness of local governments, I selected two types of policy outputs. One was the implementation of institutional mechanisms attuned to minority grievances. Because discriminatory police practices were ranked by most ghetto residents as their number one grievance in a survey of riot cities conducted by the Kerner Commission (1968, pp. 143–50), I selected three indicators of police responsiveness. These were whether a city had set up a citizen's complaint board, empowered it with investigatory authority, and implemented a police-community relations program. The first two indicators may serve as measures of official willingness to hear and take action on specific grievances filed against the conduct of police personnel. The latter may represent the extent to which local officials are concerned about the mode or style of law enforcement in the black community (Wilson, 1969).

The second type of policy response dealt with the financial commitment of urban officials to the social needs of disadvantaged citizens. Here I selected three additional indicators to measure the relative proportion of all expenditures allocated toward the vital areas of

Table 3. Correlations of Disorder Propensity and Governmental Form

Governmental Form	Disorder Propensity r_0	Disorder Propensity r_1
Manager-council government	–.15*	–.08
At-large districts	–.06	–.02
Nonpartisan elections	–.08	–.04
Reform index[a]: linear model	–.16*	.08
quadratic model (R)	.21	.13

r_0 Simple Pearson product-moment correlation.
r_1 Partialled for the proportion of nonwhite families who are poor and the proportion of nonwhite women who are divorced or separated.

[a] This index ranges from a value of 0 for forms of government combining mayor-council government, ward districts and partisan elections to a value of 7 for their reform counterparts.

$*p < .05$
$**p < .01$
$***p < .001$

health, education and welfare. The correlations between these six indicators of governmental responsiveness and the disorder propensity are tabulated in Table 4.

The lack of formal grievance mechanisms against alleged police misconduct and brutality has long been a key issue among urban blacks. Nevertheless, neither the existence of a citizen's complaint board nor the power of citizen investigation is significantly related to the incidence of racial violence. Black rioting during the latter 1960s also appears to be unrelated to the existence of a police-community relations program.

The alleged inadequacy of health, education and welfare services is another issue which has been emphasized in riot commission reports (Kerner, 1968, pp. 251–77). Because of data limitations, only crude tests of the hypothesized relations between the distribution of these services and the incidence of racial strife were possible in this analysis. The relative proportion of the city budget allocated for a given service does not adequately reflect either its quality or distribution among different urban groups. Moreover, it should be remembered that education and welfare service levels are largely dictated by decisions made at the state, county and district levels of government. For these reasons, the results reported here should be weighed with some caution. Notwithstanding, it seems reasonable to infer that racial disorders were not a response by urban blacks to financial allocations in the city budget for human services. In fact, the data show that rioting was most likely to occur in cities where welfare expenditures were proportionally higher, even in the presence of statistical controls for standard indicators of welfare need.

A third dimension of closure theory focuses on the extent to which urban blacks experience institutional discrimination. One persisting form has been the exclusion of blacks from public service positions. Studies of ethnic assimilation suggest that minority groups frequently obtain elective positions before any other kind and usually have their greatest proportional representation in them (Meyers, 1951). By comparison, municipal employee positions are generally much more difficult to acquire because of continuing institutional barriers and prejudices against the acceptance and inclusion of minority groups and long-standing biases in the distribution of patronage. Consequently, blacks are generally underrepresented on city bureaucracies in proportion to their population size, despite pressures by civil rights leaders on local authorities.

Since the inception of the civil rights movement, two institutions which have come under increasing criticism for their exclusionary hiring practices are the police department and the public school system. The emphasis on these two institutions may be understandable, since police officers and teachers are among the most important and visible representatives of political and institutional authority. Because most urban disorders constitute a defiance of civil authority, both police officers and teachers frequently find themselves in the first line of defense. In addition, these two institutions exert perhaps the greatest control over the daily activities of urban blacks, particularly young males who constitute the majority of riot participants.

Three other forms of institutional discrimination which appear to have high racial salience are residential segregation, school segregation

Table 4. Correlations of Disorder Propensity and Governmental Responsiveness

	Disorder Propensity	
Governmental Responsiveness	r_0	r_1
Citizen's complaint board	.04	–.04
Citizen's investigatory power	–.07	–.11
Community relations program	.11	.03
Percent educational expenditures[a]	–.12	.09
Percent welfare expenditures[b]	.25*	.25*
Percent health expenditures	.03	.10

r_0 Simple Pearson product-moment correlation.

r_1 Partialled for the proportion of nonwhite families who are poor and the proportion of nonwhite women who are divorced or separated.

[a] Data for 87 cities were not available because education is funded by separate school districts.

[b] Data for 65 cities were not available because welfare is funded by the county level of government.

*p < .05
**p < .01
***p < .001

and job discrimination. These conditions have also been linked to a variety of social and political problems peculiar to the black condition, namely, the systematic exclusion of blacks from the primary labor market (Doeringer and Piore, 1975), their progressive educational failure in the schools (Coleman et al., 1966; Jencks et al., 1972), and ultimately, their exclusion from positions of political power and authority (Matthews and Prothro, 1966; Keech, 1968; Bachrach and Baratz, 1970).

Table 5 presents the correlations between the disorder propensity and six indicators of institutional discrimination. The minority representation ratios were included to measure the extent of black representation on the police force and public school faculties. I computed each ratio by dividing the proportion of police officers or teachers who are nonwhite by the proportion of the total population or students in the school system who are nonwhite. Ratios less than one indicate that blacks are underrepresented. Ratios greater than or equal to one indicate that blacks have more than their proportional share of positions or are perfectly represented. For the two segregation variables, a value of zero indicates that the proportion of blacks living in each census tract and the proportion of black students attending each of the city's public schools are equal to their citywide concentrations.[11] A value of one indicates the total segregation of the races.

The results appear to provide greater support for the *cultural contact-conflict hypothesis* than the deprivation hypothesis. While the deprivation hypothesis correctly predicts that the disorder propensity will be positively correlated with the unemployment ratio and negatively correlated with the minority teachers ratio, it is unable to explain why the disorder propensity is also negatively correlated with the index of school segregation. By contrast, the cultural conflict hypothesis is able to account for the behavioral impact of all three indicators consistently. Thus, racial violence may be more likely to occur in cities with high unemployment ratios because unemployment is an issue with high racial (cultural) salience (Campbell and Schuman, 1968; Feldstein, 1973). Violence may be less likely in cities where schools are more racially segregated because racial separation provides fewer opportunities for interracial (cultural) conflict. Conversely, violence may be more likely to occur in racially changing cities

Table 5. Correlations of Disorder Propensity and Institutional Discrimination

	Disorder Propensity	
Institutional Discrimination	r_0	r_1
Minority police ratio[a]	–.13	–.08
Minority teachers ratio	–.40***	–.24**
Unemployment ratio[b]	.29***	.18*
Unemployment differential[c]	.17*	.09
School segregation[d]	–.33***	–.24**
Residential segregation[d]	–.26	–.20

r_0 Simple Pearson product-moment correlation.

r_1 Partialled for the proportion of nonwhite families who are poor and the proportion of nonwhite women who are divorced or separated.

[a] Data for 40 cities were not available.

[b] Ratio of the nonwhite to total unemployment rates.

[c] Difference between the nonwhite and white unemployment rates.

[d] Gini index.

$*p < .05$
$**p < .01$
$***p < .001$

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[11] Gini indices of residential segregation were obtained from Taeuber and Taeuber's *Negroes in Cities* (1965). Gini indices of school segregation were computed from data published in the 1967 *Elementary and Secondary Education Directory* (1969). To compute the school segregation indices, I first enumerated all of the public high schools within each city and then ranked them according to the respective proportion of nonwhite students within each school–that is, from high to low nonwhite concentrations. Next, I computed the cumulative distribution functions for white and nonwhite students. Finally, school segregation indices were computed by numerical quadrature integration from the formula given in n. 9.

where the proportion of black faculty lags behind the proportion of black students, because the interracial contact of white teachers and black students provides more opportunities for racial (cultural) conflict (Armor, 1972).

Finally, it is within a perspective of cultural contact-conflict that the positive correlation observed earlier between the disorder propensity and welfare expenditures may best be interpreted. That is, welfare benefits and services may enhance the opportunities for racial (cultural) violence by "liberating" black clients from the social norms and organizational restraints of gainful employment.

To conclude this section, Table 6 presents the results of a stepwise regression analysis for selected indicators of black well-being, social marginality and institutional closure. The data show that the political environment of blacks influenced the incidence of racial violence independently of economic and social conditions. Collectively, the variable indicators explain over 43 percent of the variation in the disorder propensity. These results appear to differ with several studies which find that recent urban riots had little to do with the black social and political condition (Spilerman, 1970; Jiobu, 1971; Morgan and Clark, 1973).

3. A Developmental Synthesis

The results of this analysis support the conclusion that the likelihood of urban racial violence is significantly related to the social condition of urban blacks, the degree of social disorganization within the black community and the extent to which a city's institutional structure is open or closed. The overall configuration of the conditions which predict racial violence appear to parallel the general contours found for protest behavior (Eisinger, 1973, p. 26). That is, violence seems most likely to occur in cities with a mixture of open and closed characteristics.

Table 6. Cumulative Explained Variance in Disorder Propensity

Variable Indicators[a]	Cumulative Variance
Black Well-Being	21%
Social Marginality	32%
Institutional Closure	43%

[a] The stepwise analysis only employed those indicators for which there were complete data. The reported results thus apply to eight indicators of black well-being, eleven indicators of social marginality and twelve indicators of institutional closure.

Thus, racial violence is significantly greater in cities which allocate larger proportions of the total budget for welfare services, where the social status of blacks approaches that of whites, and where blacks are less racially segregated. These conditions are characteristic of opportunity structures which, if not generically open, certainly appear more responsive and receptive to black needs and aspirations than not. At the same time, the results suggest that racial disorders are more likely to occur where certain features of a city's opportunity structure are manifestly closed. Consistent with closure theory, the likelihood of racial disorders was observed to be measurably greater in cities which failed to recruit black teachers in proportion to the concentration of black students within the school system. Racial strife was also greater in cities with high reported levels of social disorganization and job discrimination.

How are these two sets of seemingly incongruent relationships to be explained? And what is the true character of urban racial violence? Is it the inevitable flotsam and debris of an urban society which has run aground on the shoals of social disorganization and the acquisitive behavior of "riff-raff" elements? Or does it represent, instead, outbursts of righteous indignation by politically powerless rebels against the more visible and salient forms of institutional and social oppression?

The results of this study and their seemingly paradoxical implications certainly afford fertile ground for continuing debate between these two contending schools. Nonetheless, the results are not necessarily inconsistent, nor do they foreclose rational interpretation. One point of departure for this task is classic development theory. Figure 2 presents a developmental model of disorder propensity which is premised on the assumption that the nation's cities can be ranked on a scale of black political development, ranging from a low of social lethargy and quiescence to a high of active participation and involvement in the political process (Huntington, 1968; Eyestone and Eulau, 1968; Winham, 1970; Sharkansky, 1975). Black political developmental itself is assumed to vary primarily as a function of black economic development (Lerner, 1958; Dahrendorf, 1959; Apter, 1965; Huntington, 1968).

The model depicts the disorder propensity as a curvilinear function of black social class. This has been done to conform with the relationship observed between civil violence and social class at the cross-national level (Alker and Russett, 1964, pp. 306–07; Fierabend et al., 1967, pp. 654–57; Midlarsky and Tanter, 1967, p. 215;

Huntington, 1968, pp. 39–53), to integrate the results of this study into the model, and to emphasize major transitional stages of black political development. Each stage is assumed to become ascendant or modal when blacks as a group attain specified levels of material well-being. These are denoted in the diagram as thresholds of developmental mobilization.

The threshold of economic mobilization is assumed to separate black apathy and social quiescence from collective social violence.[12] In the United States, this demarcation may correspond to the difference in the black condition between southern and northern cities. While black living standards are surely more affluent in the North, as measured by a number of class indicators, this improvement in well-being may have been achieved at the expense of significant social costs (Olson, 1963, p. 532). The severing of kinship and cultural ties, the social dislocations of migration, and the economic strains of life in a new urban environment have all been advanced to explain the higher incidence of family breakdown and community disorganization among northern, as opposed to southern blacks (Moynihan, 1965). As urban historians like Zane Miller (1973, pp. 45–51) have suggested, the parallels between the socially debilitating consequences of modernization between southern blacks and European ethnic whites 100 years earlier are, indeed, striking. Both migratory waves to northern cities were products of economic mobilization and development. Both culminated in unprecedented levels of social disorganization, ethnic unrest and civil violence.

The threshold of social mobilization, by comparison, may demarcate the division between Maslow's (1954) lower-order (more fundamental) needs of survival and security and the higher-order needs of self-esteem and self-actualization (Huntington, 1968, pp. 32–39, 47–59). Levels of black well-being which fall below the social threshold but above the economic threshold are assumed to be more conducive to social violence. Levels which fall above the social threshold but below the political threshold may be more likely to activate blacks in collective political violence and protest demonstrations.

Hence, the continued economic progress of blacks during the 1960s (Wattenberg and Scammon, 1973) may have been the needed social catalyst which ignited the civil rights movement and fueled the conflagration of the cities. In political terms, the black urban riots and the civil rights movement have been interpreted as attempts to eliminate the racial barriers of discrimination and segregation, the former through the politics of violence, the latter through the politics of nonviolent protest (Silver, 1968; Skolnik, 1969; Nieburg, 1969; Fogelson, 1971; Feagin and Hahn, 1973). In developmental terms, they may be viewed as social mobilizations of self-conscious blacks who sought to secure equal social status with whites and equal opportunities for social advancement. Under conditions of social mobilization, ghetto blacks may be more likely to develop needs for self-esteem and self-actualization, to reject old racial stereotypes and symbols of racial subservience, to translate their social freedom into militant attitudes, and to participate in collective violence and protest. Political violence, like protest, is a strategy of the politically powerless (Lipsky, 1968). It may most often be employed when blacks acquire the aspirations for social justice, but lack or are denied the organiza-

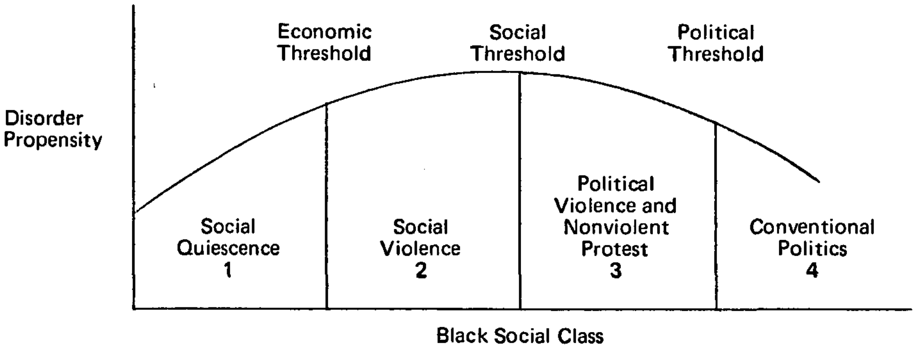

Figure 2. A Developmental Model of Disorder Propensity

tional resources and political access for achieving it.

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[12] The term, *social violence*, is used to connote all forms of crime and civil violence in which the targets selected for attack are neither political nor institutional in character.

The threshold of political mobilization denotes that point of development when urban blacks begin to acquire on a much broader scale the benefits of active involvement in the political process. At this stage their increasingly middle-class social status is much more supportive of voting and other forms of political participation. Second, their increased voting power and the political candidacy of middle-class black "ethnics" on the ballot assures them greater representation on the councils of city government (Wolfinger, 1965; Pomper, 1966), greater representation in municipal agencies (Meyers, 1951, pp. 143–44), and a real measure of political power (Rustin, 1966), as reflected, for example, in their ability to elect black mayors (Hadden et al., 1968) and alter the structural biases of existing urban institutions (Stinchcombe, 1968).

In sum, the model implies that violence may be least where life is a constant battle to satisfy basic physical needs or where affluence and an open political structure facilitate black political participation and the institutional resolution of black grievances. Violence may be greatest where blacks suffer the dislocations of social disorganization and where their demands for social and political change are thwarted by a lack of political resources or a closed institutional structure. This region of collective social and political violence is depicted in Figure 2 as lying between the thresholds of economic and political mobilization. To adopt Daniel Lerner's (1958) terminology, the curvilinear model assumes that racial violence is greatest among "transitional" urban blacks, least among their "traditional" and "modernized" counterparts.

The results of this study, at least, lend credence to a developmental interpretation of the black urban riots.[13] Consistent with the logic of economic mobilization, the incidence of racial strife was found to increase with higher levels of family breakdown and social disorganization in the black community. Consistent with the logic of social mobilization, racial violence was also found to increase as urban blacks progressed above the poverty level in income, approached whites in social status, and came into increasing cultural conflict with whites in the community and the schools. Finally, consistent with the logic of political mobilization, violence was observed to decrease as blacks gained access to employment opportunities in the public and private labor markets.

While these results support a developmental explanation, the ultimate test of any model is its empirical fit with the data. To test the model in Figure 2, I factor analyzed three measures of education, occupational status and income among urban blacks to obtain a generalized measure of black social class. These are: (1) the proportion of nonwhites who had received at least a high-school education, (2) the proportion of nonwhite males who were employed at the crafts-foreman level or higher, and (3) the proportion of nonwhite families with annual incomes above the poverty level. The results of a principal component analysis revealed that a single weighted average explained over 72 percent of the total variation in the three measures. Then I fitted a quadratic function of this generalized class measure to data on the disorder propensity by means of regression analysis. The computed regression coefficients and corresponding significance levels are tabulated in Table 7. A scattergram of the regression curve and the data is plotted in Figure 3.

The results of this analysis conform to the predicted curvilinear relationship between the disorder propensity and black social class. Racial violence first increases with improving black well-being and then declines in a convex parabolic fashion. Moreover, both the quadratic and linear coefficients are statistically significant. This implies that the curvilinear model provides a better fit to the data than a simple linear model.[14]

Although these results provide additional support for a developmental interpretation, I decided to pursue the test of this paradigm one step further. Table 8 presents a breakdown of six developmental indicators and the mean disorder propensity[15] by four rank levels of black social class. The developmental indicators

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[13] Since the data are cross-sectional, it is implicitly assumed here that the developmental process is essentially invariant across time, namely, that cities with high levels of black political development may be temporally treated as posterior representatives of cities with low levels of black political development. While the assumption of an invariant developmental process is basic to the modernity literature, it may not always be justified (Marquette, 1974).

[14] Although a curvilinear model provides the best fit to the factor-analyzed measure of black social class, it does not improve the fit for each of the three selected indicators.

[15] The mean disorder propensity was computed on the basis of the regression coefficients tabulated in Table 7 and the factor scores of black social class.

Table 7. Curvilinear Regression Analysis of Black Social Class

Independent Variables	Regression Coefficient	Significance
$(\text{Black Social Class})^2$	–.0186	$p<.05$
Black Social Class	.0755	$p<.05$
Constant	.0869	
	R = .25	

were selected for their presumed relevance to mobilization theory. The social ranks were computed by dividing the black social class variable at the value which maximizes the curvilinear function and then subdividing both interval segments at the midpoints between this value and the minimum and maximum values.[16]

In terms of the developmental profile, F_1 represents the grouping of 42 cities in the sample which may be classified as "socially quiescent." Similarly, F_2, F_3 and F_4 represent the respective subsamples of 58, 13 and 6 cities which fall into the more developed stages of "social violence," "political violence and protest," and "conventional politics." The three successive cutpoints between these divisions represent the hypothesized thresholds of economic, social and political mobilization.

Because of the crudeness of the selected mobilization indicators and the difficulty in selecting suitable cutpoints, the results of Table 8 should not be expected to delineate a pure modernity profile. Notwithstanding these limitations, the data provide surprising support for the model's developmental assumptions. Thus, as the model predicts, regional location and conditions of social disorganization appear to demarcate those cities in the sample which have experienced the socially destabilizing effects of black economic mobilization from those which have not. For example, 74 percent of the cities which fall into the economically underdeveloped stage (F_1) are southern. This compares with 81 percent in the economically mobilized stage (F_2) which are northern. Similarly, the data predictably show that the disintegration of the nonwhite family structure is the most advanced in the economically mobilized stage.

The two tabulated indicators of social mobilization were selected on the assumptions

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[16] These selected cutting points may not necessarily be optimal, since it is difficult to discriminate among the four hypothesized stages of black development on the basis of class considerations alone.

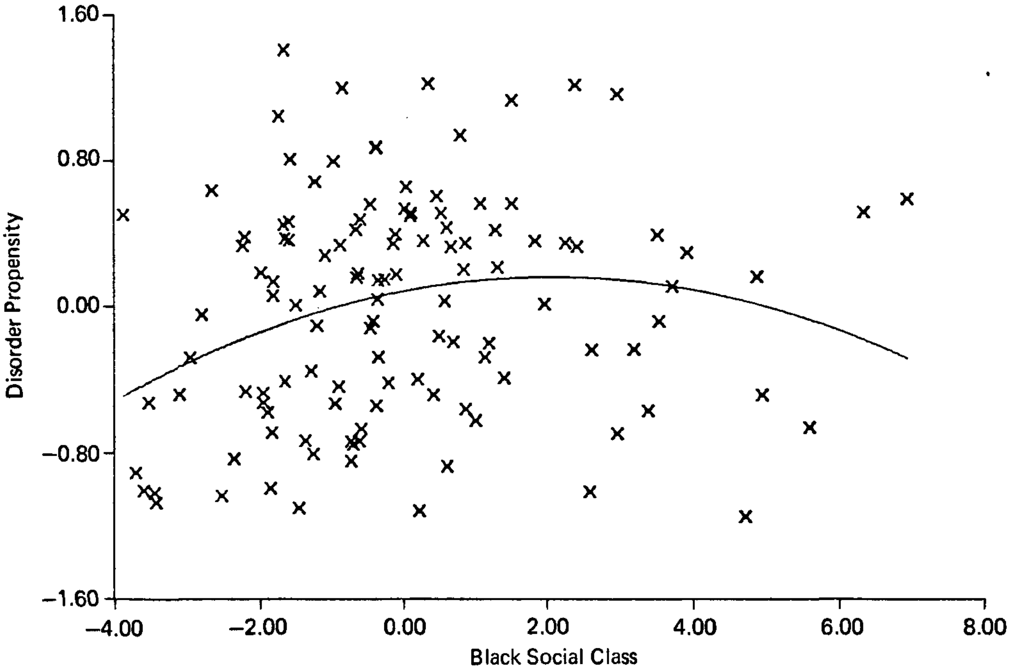

Figure 3. Scattergram of Disorder Propensity and Black Social Class

that: (1) the threshold of social mobilization may be approximated by the subsistence definition of poverty,[17] and (2) school desegregation is more likely to occur under conditions of social mobilization. Consistent with this interpretation, the data show that the highest levels of reported racial strife are to be found in cities where substantial majorities of the black community have crossed the poverty threshold. At this point of development (F_2-F_3), the greater social mobilization of the black community may also be reflected in the sharply declining levels of racial segregation in the schools.

As indicators of political mobilization, the relative representation of blacks on the police force and the unemployment differential may leave something to be desired. This is because both measures also reflect the greater employability of blacks by virtue of their increasingly middle-class social status. In the absence of more appropriate indicators, however, they may serve as indirect measures of black success in eliminating the vestiges of racial discrimination in the public and private job markets. If this assumption is made, then it would appear that urban blacks make their greatest employment gains under conditions of full economic development. It is also at this juncture (F_3-F_4) where racial violence appears to lose much of its strength and potency as a vehicle for expressing black discontent.

Up to this point, the results of this study have been presented for a national sample of 119 cities. No separate breakdown has been made in the analysis for northern and southern cities. As noted earlier, however, several recent studies in the sociological literature have attempted to show that most socioeconomic and political conditions of the central city have little to do with the reported incidence of racial strife once controls are introduced for the nonwhite population size and region (Spilerman, 1970; Jiobu, 1971; Morgan and Clark, 1973). Although the findings of this study have been shown to hold in the face of a statistical control for the nonwhite population size, might different results obtain if an additional control were added for regional location?

To check this possibility, I made a regional breakdown of the correlations between the disorder propensity and representative predictors of racial violence for which there were complete data. The results are presented in Table 9. In addition, I have displayed the correlates of a dichotomized regional variable (South = 0; North = 1) in order to show the variation in these riot predictors with regional location. As can be seen, most of the disorder propensity correlates are statistically nonsignificant. Contrary to the conclusions of the sociological literature, however, this result does not imply that socioeconomic and political conditions of the central city had little to do with the outbreak of the black urban riots. As the data in Table 8 have already demonstrated, regional location is not a trivial indicator. Rather, it appears to be an important predictor of black political development. That is, it distinguishes cities where black communities are in an arrested stage of economic development from those cities where blacks have made substantial progress. And as the regional correlates in Table 9 confirm, region is also a

Table 8. Breakdown of Developmental Thresholds, by Black Social Class

	Black Social Class			
Thresholds of Mobilization	F_1	F_2	F_3	F_4
Economic:				
Region	.26	.81	.85	1.00
Percent NW women divorced or separated	16.8	17.6	16.0	11.7
Social:				
Percent NW families poor	51.0	34.8	25.2	18.5
School segregation	.68	.55	.47	.35
Political:				
Minority police ratio[a]	.36	.75	(2.33)	
Unemployment differential	4.4	5.6	3.0	2.2
Mean disorder propensity	–.16	.09	.14	–.08
N	42	58	13	6

[a] Due to missing data, the cell sizes for F_1, F_2 and the grouped stage, (F_3, F_4), are respectively 30, 38 and 11.

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[17] See n. 8.

crucial indicator (74 percent explained variance) of the social and political conditions which predispose northern cities toward greater racial violence than southern cities. Hence, it would appear that the sociological literature has begged an important theoretical issue; i.e., the empirical significance of region. By introducing atheoretical statistical controls for regional location, sociologists may also have largely obscured the developmental implications of their data.

To conclude, the findings of this study provide a strong basis for rejecting the premise that the recent black urban riots had little to do with the social and political environment of central cities. While they do not support a progressive deprivation thesis, they are consistent with a developmental interpretation. Hence, collective racial violence appears to be a form of social and political activity which marks the upward social ascent of urban blacks. At the same time, it may also be an indicator that their collective climb for social and political equality will be long and difficult.

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Table 9. Regional Breakdown of Results for Selected Indicators of Black Well-Being, Social Marginality and Institutional Closure

	Regional Breakdown		
		Disorder Propensity	
Variable Indicators	Region	South	North
Percent NW families poor	–.73***	–.09	.16
Personal income inequality	–.73***	.09	–.15
Percent NW women divorced or separated	.17*	.12	.23*
Police density	.23*	.19	.13
Minority teachers ratio	–.73***	.01	–.13
School segregation	–.54***	–.25*	–.04
R^2	74%	20%	13%
N	119	44	75

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Toward Applicable Social Choice Theory: A Comparison of Social Choice Functions under Spatial Model Assumptions*

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This article develops a formal framework to aid political designers in the comparison of social choice functions. It generalizes earlier assumptions of "impartial culture" so that we may begin to investigate the effect of politically interesting variations on the probability that different social choice functions will satisfy given performance criteria. As an application of the framework, a detailed Monte Carlo study compares the ability of four different social choice functions to select a Condorcet winner when voter preference orders have been generated from a spatial representation of ideal points and alternatives. We also investigate the potential of alternative methods of selecting winners in presidential primary elections.

Political designers have made systems of voting one of their major concerns at least since the lawgivers of classical Greece. The choice of a voting system may have many different kinds of consequences. Its operations may satisfy some notions of fairness and violate others. It may bias social choices toward or away from the status quo or the interests of particular groups. It may create or suppress information that affects the subsequent evolution of preferences within the polity. It may be more or less resistant to various classes of manipulations. Since the Enlightenment the use of logical and mathematical tools to investigate some of the important consequences of voting systems has grown steadily. Since World War II it has grown explosively. As a result we now have a substantial body of rigorously derived results about voting systems that may be of some relevance to political designers.

Standing back from the detail, we can summarize the contemporary literature by saying that for every possible voting system there is at least one undesirable thing that can happen. Generally there are many. The ultimate consumer of such theoretical results is presumably the political designer. But what use can one make of them? If all plausible systems are flawed, then one must begin to ask which flaws are acceptable in a given context. That question has in its turn two components: (1) In the context for which one is designing, how important is a given flaw of a system? (e.g., that voters may gain by misrepresenting their true preferences when they cast a ballot). (2) How likely is that flaw actually to be exhibited in the context? (perhaps the voters in question will typically have very little of the information about how others will vote that is required to profitably misrepresent true preferences). Our goal in this article is to take some first steps toward enabling political designers to obtain answers to at least some important questions of the second type.[1]

Our effort to sketch the requirements of making the modern social choice literature more useful to political designers can be simplified by considering a hypothetical voluntary organization that elects a new president each year by the *plurality* method. (The winner is the candidate who is the first choice of the largest number of voters.) We will suppose that in the group's current election there are four candidates, whom we shall label A, B, C and D. The winner is B with 1/3 of the votes, A and C each get 1/4, and D trails with 1/6. After the election, supporters of the losers are dissatisfied, not because they believe the votes were unfairly cast or counted, but because they feel that somehow the plurality system has not produced the "right" result.

Supporters of D argue that B is the choice of an intense minority, but lacks the kind of broad support that, they are sure, D does in fact command. Supporters of A are practically positive that in a straight runoff their candidate would have defeated B. They maintain that the votes of C and D supporters have been unfairly wasted since those voters had a preference for A over B that the plurality system could not take into account. The supporters of C argue that it is not good for the group to have forced upon it a candidate who is intensely disliked by a large block of members. They feel sure this is true of B (and probably also of A).

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[1] Used in this manner, social choice theory is an important example of what Ranney (1976, p. 140) has called "political engineering"; namely, "the application of empirically derived general principles of individual and institutional behavior to fashion institutions to solve practical political problems." The promise of social choice theory in this context is considerable, but, as we argue below, its fulfillment must await an adequate base of naturally occurring preference data.

All of these positions could, of course, be discounted as "sour grapes." Indeed, this is what generally happens in such situations, and it is all the more likely to happen in view of the fact that plurality elections do not generate the data which would allow one to investigate the losers' concerns. The fact is that even so diverse a group of complaints and conjectures as the ones laid out above need not be logically inconsistent. If we could induce the voters in our hypothetical organization to rank-order the candidates for us, we might get a set of numbers such as those shown in Table 1. For each of the 4! = 24 possibilities it gives the fraction of the electorate ordering the alternatives in that particular way.[2] The numbers shown generate the plurality outcome we have reported.

We can now point out that each of the objections discussed above is the kernel of an argument for an established alternative method of counting the votes. When we develop the method corresponding to each argument and apply it to the election in Table 1, each group of unhappy losers turns out to have been correct. That is, for each group, if the votes in this election had been counted according to the method it saw as fairest, its candidate would have been the winner.

The argument of D's followers that the winner should be the candidate with the broadest total support leads to the *Borda Rule*, the method which selects the candidate with the highest average ranking in the set of ballots cast. A long-established response to the "wasted vote" problem is the method advocated by *Hare* in which the rank-orderings are used repeatedly to produce what is in effect a sequence of plurality elections in which the last-place finisher is eliminated at each stage until a winner emerges. The argument of C's supporters leads to a variant on the Hare method first proposed by the mathematical psychologist Clyde Coombs. In the *Coombs* system the winner emerges from a sequential process in which at each stage the rank orderings are used to find and eliminate the candidate most frequently ranked last among the alternatives remaining, until some candidate achieves a majority of first-place votes.[3]

The numbers in Table 1 are what we will refer to as an *election*, the relative frequencies in the electorate of support for each possible permutation (rank ordering) of the alternatives. Each method of election (social choice function) has been evaluated for this hypothetical election and the results are displayed in Table 2. (The appendix contains detailed mathe-

Table 1. Hypothetical Election

Preference Order	Relative Frequency	Preference Order	Relative Frequency
ABCD	.0417	CABD	.0000
ABDC	.0000	CADB	.0000
ACBD	.0417	CBAD	.0000
ACDB	.0000	CBDA	.0000
ADBC	.0000	CDAB	.1250
ADCB	.1666	CDBA	.1250
BACD	.0000	DABC	.0833
BADC	.0000	DACB	.0833
BCAD	.0000	DBAC	.0000
BCDA	.2291	DBCA	.0000
BDAC	.0000	DCAB	.0000
BDCA	.1042	DCBA	.0000

Table 2A. Plurality and Borda Voting

Candidates	Plurality[a]	Borda[b]
A	.2500	2.78
B	.3333*	2.77
C	.2500	2.30
D	.1666	2.23*

[a]Percentage of first place votes.
[b]Average rank (1=first, 4=last).
*Designates winning candidate.

Table 2B. Order of Elimination for Hare and Coombs

Candidates	Hare	Coombs
A	winner	first
B	third	second
C	second	winner
D	first	third

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[2] There are 24 transitive orderings on four candidates when no indifference between candidates is permitted. While the method of analysis we employ can be straightforwardly adapted to include indifference, to do so greatly complicates the presentation of the model without changing the nature of the results. For this reason we have not considered the possibility of indifference in our analysis.

[3] Discussions of these social choice functions can be found in the references listed in the appendix.

matical definitions of our concept of an election, of the space of possible elections, and of the four social choice functions we are considering.) Table 2 indicates that each of the four social choice functions does in fact select a different winner in this particular election. It is a stark demonstration that the choice of a system for counting the votes can make a difference. But given that the choice may be consequential, how is our group to choose its social choice function?

That choice is very complicated, since it depends upon a host of factors, each of which is plagued by substantial uncertainties. We have given a number of arguments against plurality. Against Hare and Coombs one can argue that sequential elimination requires detailed procedural decisions that may affect the outcome and be hard to explain to the electorate. Against Borda it has been said that there may be serious incentives for voters to misrepresent their preferences, for example, ranking a true second choice last on the ballot to prevent it from "sneaking" past the most preferred alternative. Many more such concerns could be adduced, and for each we could ask, "How often is this problem likely to arise?" Social choice theory probably cannot hope to settle questions of their relative importance, since this will vary from one context to another, but it can—we think—be extended to help reduce the uncertainty surrounding many of them. The remainder of this article develops a methodology for answering questions on the frequency with which the possible defects of alternative social choice functions will arise in a particular context.

We will illustrate the method by considering one possible serious defect of a social choice function. That defect is the failure to satisfy the criterion first proposed by *Condorcet:* if there is a candidate who would defeat every other candidate in simple pairwise comparison, that candidate should be selected. As the famous paradox of voting shows, Condorcet winners do not always exist, but in the literature on social choice there is fairly wide agreement that very frequently they do exist, and that when they do they should be the social choice. In the particular election we have discussed, a Condorcet winner exists and happens to be C, the choice under the Coombs system. Unfortunately, this gives no valid grounds for preferring Coombs to the other three possible systems, since Coombs will not always choose the Condorcet winner when the systems disagree. If our hypothetical group puts substantial weight on the Condorcet criterion as an indicator of a good social choice function, we need to be able to answer a much more difficult question: how frequently would each social choice function miss a Condorcet winner *in organizations like this one?* The method we develop below will allow us to determine for an abstract model of a particular political context how often to expect a Condorcet winner and how often an existing Condorcet winner will be missed by each of the four social choice functions we have been considering. It will thus make it possible to advise our group about this aspect of their choice.

The approach we are taking makes it possible to illuminate the performance of given social choice functions on many other criteria of interest (e.g., consistency, nonmanipulability, or competitiveness), but for the most part we will concentrate on this one dimension of evaluation, the frequency of missing Condorcet winners. We will digress briefly to discuss the competitiveness of congressional elections and alternative vote counting methods for presidential primaries.

To obtain such performance evaluations for social choice functions we will need to define three main concepts: election, election space, and a probability density on election space. An *election* is simply the relative frequency in an electorate of each of the $k!$ possible rank orderings of k candidates or alternatives. The 24 numbers in Table 1 therefore constitute an election in our sense with $k=4$. An *election space* is the set of all possible elections that could occur when there are k alternatives. It corresponds to all the points in a space of $k!$ dimensions that have no negative coordinates and have coordinates summing to unity.[4] The virtue of defining things in this fashion is that many events of interest to political designers—such as, "The Condorcet winner is missed by the Borda rule"—correspond to well-defined (and mathematically well-behaved) regions of election space.[5] It then becomes possible to restate the concerns of the political designer as follows: "What is the probability that the system for which I am designing will generate elections in the region(s) corresponding to the event(s) in which I am interested?" To answer such a question one needs a *probability density on the election space* that will establish for the given design context the probability that elections will occur within any particular region of interest.

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[4] This structure is called the $k!$-dimensional simplex.

[5] These regions in fact are convex and correspond to the feasible regions of linear programs. See Chamberlin and Cohen (1978).

It would be ideal to present an illustration for the 4-candidate case we have been developing, but that would require a 24-dimensional picture. We can, however, provide a nice illustration for a two alternative case, and it is one that permits us to exhibit the flexibility of our method by treating a different kind of event than the missing of a Condorcet winner—namely, seat competitiveness in U.S. congressional elections.[6]

When there are two alternatives—for example the candidates of the Democratic and Republican parties—an election is just the relative frequency of preference for each of the two possible rank orderings. Election space has 2! = 2 dimensions, and, as shown in Figure 1, the requirement that the coordinates be non-negative and sum to one means that elections (as we have defined them) are only those points lying on the heavy diagonal line. The labeled points vary from an election in the upper corner that is a complete victory for a Republican candidate (0, 1.0) through intermediate outcomes down to an election in the lower right corner that is a total victory for a Democrat (1.0, 0). We are now able to picture within our framework a question posed frequently by analysts of congressional elections: "What is the probability that a congressional seat will be competitive—i.e., not safe?" Traditionally seats have been classified as competitive if they were won in elections lying in the region between (.60, .40) and (.40, .60). We can obtain an estimate of the fraction of congressional elections that will fall in that region from data on the 1970 congressional contests. They lead to the probability density pictured in Figure 2, where the share of the total area under the curve that lies between the dashed lines gives the fraction of all seats that are competitive. Students of seat competitiveness have noted that, after a long period of relative stability, this fraction has recently been declining. They have begun an effort to explain the changing shape of the probability density for congressional elections (Cover, 1977; Ferejohn, 1977). We do not attempt here to make a substantive contribution to that inquiry, but merely to show that questions about a criterion like seat competitiveness may also fit quite naturally into the election space framework that we propose.

Equipped with these concepts of election, election space and probability density on election space, we can return now to our hypothetical voluntary group. If they are concerned to learn something about the performance with respect to the Condorcet criterion of the four different social choice functions, what help can we give them? The most prominent efforts to provide some help have used a probability density on election space derived from what has been called the "impartial culture" assumption. It treats each voter as a sample point drawn from an infinite population in which all possible rank orderings of the alternatives are equally likely. This approach has been used to investigate the frequency with which Condorcet winners will exist and to examine properties of other social choice functions.[7] Unfortunately

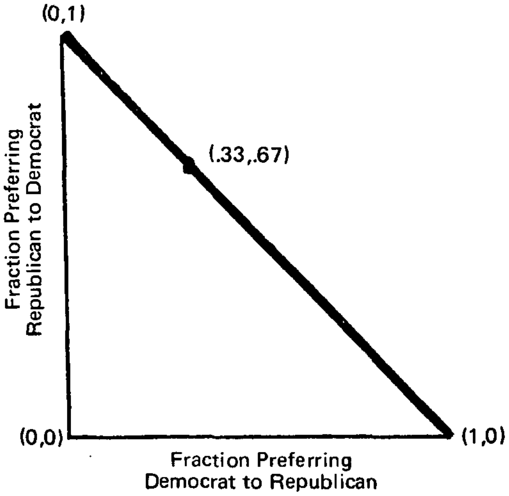
(0,1)
(.33,.67)
Fraction Preferring Republican to Democrat
(0,0)
(1,0)
Fraction Preferring Democrat to Republican

Figure 1. Election Space With Two Alternatives

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[6] In two-candidate elections a Condorcet winner obviously exists, and will be selected by all four social choice functions we are examining. We choose this case because it nicely illustrates the concept of a probability density on elections.

[7] Estimates of the frequency of occurrence of Condorcet winners have come from three sources. Investigations of voting in legislatures and university faculties have uncovered a number of instances of the absence of a Condorcet winner and other voting anomalies, but these case studies cannot provide accurate estimates of the frequency of occurrence of such events. See Riker (1958, 1965), Blydenburgh (1971), Farquharson (1969, pp. 52–53), Bowen (1972), Niemi (1970) and Michael Taylor (1968). A large number of Monte Carlo simulation studies have provided estimates of the frequency of occurrence of Condorcet winners and the performance of various social choice functions under the impartial culture assumption. Among these are Campbell and Tullock (1965), Klahr (1966), Fishburn (1971, 1974a, 1974b), Fishburn and Gehrlein (1976) and Ludwin (1976). In

this is equivalent to assuming that all alternatives are equally attractive. Thus in the competitiveness example discussed above, an impartial culture approach, combined with the large size of congressional districts, leads to a probability density so concentrated near (.5, .5) that virtually all seats would be competitive. If there are four alternatives and a very large electorate, the impartial culture approach leads to a frequency of Condorcet winners of .8245 (Niemi and Weisberg, 1968, p. 322). The probability density we will develop below leads to a much higher estimate.

These differences illustrate the necessity of deriving performance evaluations of social choice functions from a probability density on election space that is appropriate to the design context in question. The impartial culture approach amounts to assuming that the principal variable of interest is the differing levels of sampling variation associated with samples of different sizes drawn from a uniform population. The partial culture assumptions used to date have involved populations deviating from uniformity in relatively simple ways. Observation suggests, however, that in many political situations voters' preferences have a more elaborate structure that should be taken into account. The election of Table 1 indicated that our hypothetical voluntary organization may be such a case and that the partial and impartial culture models used to date may be inappropriate. How might we go about determining probabilities in a manner better suited to richer political contexts? To do so we must overcome a number of difficulties. These may be discussed in three clusters: empirical, theoretical and analytic.

Despite years of intensive empirical research on elections, we remain virtually without clues as to the likely character of the probability density of elections occurring in any electoral situation with more than two candidates. What would the density look like for our voluntary group's conditions? How would it be changed by the introduction of a nominating com-

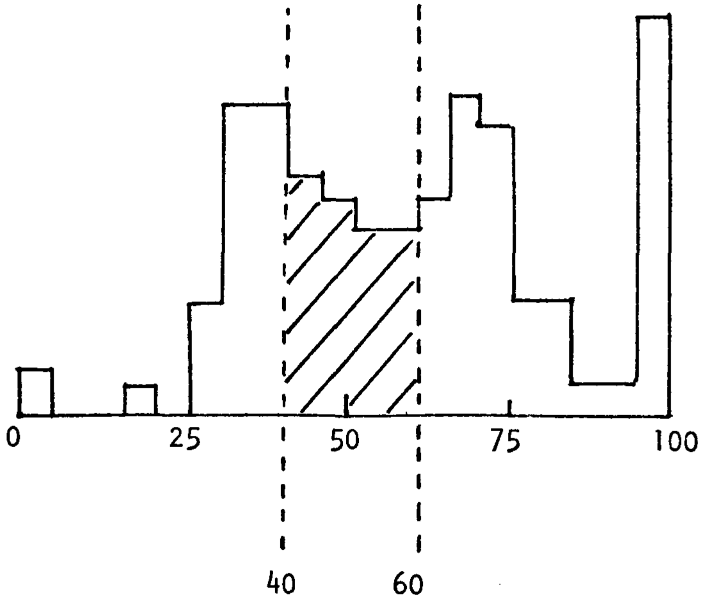

Figure 2. Histogram of Democratic Vote Share by District from the Congressional Elections of 1970 (Adapted from Tufte [1974])

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addition to considering impartial cultures, several studies have investigated voting results in a few partial cultures as well. Among these are Niemi (1969), Fishburn (1974c), Jamison and Luce (1972), Abrams (1976), and Paris (1975). In the only study to use naturally occurring data, Joslyn (1976) used survey data to investigate voting outcomes using different social choice functions. Finally, analytic solutions for the frequency of occurrence of Condorcet winners in an impartial culture have been obtained by Niemi and Weisberg (1968), Garman and Kamien (1968), and DeMeyer and Plott (1970). In addition, Kelly (1974) and Kuga and Nagatani (1974) have provided analytic solutions for several cases of partial cultures.

mittee, changes in electorate size or in voter heterogeneity? Because plurality voting (with or without runoff) is used almost universally in the United States—even in multicandidate races—we have almost no data on complete preference orders over three or more alternatives. Moreover, in most of the few cases where some form of ordinal ballot has been used, only aggregate data have been recorded. This is not sufficient to reconstruct the "election" in our sense of the term. In the medium term this lack of a solid empirical grounding for our assumptions about election densities is remediable. We are now collecting, and urging others to tell us about, data sets containing ordinal preferences.

Even when appropriate data are more abundant, there will be difficult theoretical and analytic problems. An adequate theory of the process which generates elections will have to be constructed and validated, and the probability density on the election space implied by that theory will have to be determined. That density may be quite complicated and it will have to be integrated over each region corresponding to an event of interest.[8]

We think that eventually these empirical, theoretical and analytic complexities can be unraveled so that an elegant, abstract and *applicable* comparison of social choice functions will be possible. In order to take a step immediately, however, we have cut through this triple Gordian knot with the sword of Monte Carlo, as have our predecessors in this domain. Where we differ from them is with respect to the underlying structure of the election generating process which we consider. Instead of assuming that each of the possible preference orders on a set of candidates is equally likely, or instead of making "partial culture" assumptions, we employ an explicit spatial model of voter preference. The choice of a spatial model is not an arbitrary one, for it is a well-developed and plausible theory that has served as a guide for empirical research.[9] We consider voters and candidates distributed in a multidimensional issue space, and use the Euclidian distance between a voter's optimum and each of the candidates to derive a preference order among the candidates for each of the voters. These preference orders are then aggregated into elections—that is, the resulting set of relative frequencies of the possible preference orders. Sets of elections are then samples of points from the probability density on election space and can be tested to see what fraction of the sample falls within regions of interest.

The Spatial Model. In order to use a spatial model approach to the Monte Carlo generation of elections, we will have to make a number of assumptions about the electoral situation to which our results might be applied. For example, we will need to know the size of the electorate, the number of candidates likely to be active in elections, and the number of dimensions underlying the main issues arising in the system's elections. An advantage of the spatial model, however, is that the information required is either directly available, as is usually the case for size or number of candidates, or may be determined from survey data, as would be the case for the dimensionality of the major issues over which the voters have preferences.

The set of assumptions underlying the model we will be exploring are as follows:

(1) *Size of electorate.* We will study two cases, 21 voters and 1000 voters.

(2) *Dimensionality of issues.* We will study a system in which four dimensions are adequate to describe the salient characteristics of candidates over which voters have preferences.

(3) *Differentiation of candidates.* We will study three different cases, all having four candidates: one in which candidates are about as similar to each other as randomly drawn voters would be, one in which the candidates are substantially more alike than voters, and one in which the candidates are much more differentiated than would be the case for randomly drawn voters.

Before we detail our elaboration of these basic assumptions into a spatial model of an electoral situation that can be used to generate elections, we should make clear the relationship between the generality of our method and the specificity of these assumptions. Our position is that electoral situations have many features that may be important in determining the frequency of various events of interest, and that there is high likelihood of significant interactions among those features. (For example, the size of the electorate interacts with differentiation of the candidates in the case we will be studying.) A method for obtaining applicable results therefore needs to have two components: an adequate model of the electoral situation for which one is designing, and a procedure for exercising that model to produce results about events of interest to the designers. It is desirable that the latter component be quite general so as to place as few constraints as possible upon the model. On the other hand, the model itself should be as specific as is necessary to represent the salient features of the design situation.

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[8] While the mathematical difficulties appear formidable, there is some hope. We mentioned above that many such regions are convex subsets of the $k!$ simplex. It may be possible to use advanced techniques of manipulating the system of linear inequalities that bound the region in order to get them into integrable form.

[9] The theoretical literature on the spatial model is large. Among the important contributions are Downs (1957), Davis, Hinich, and Ordeshook (1970), Hinich and Ordeshook (1970), Hinich, Ledyard, and Ordeshook (1972) and McKelvey (1975). For empirical work using this formulation, see Aldrich (1975), Aldrich and McKelvey (1977), and Rabinowitz (1973).

Thus we wish to be the first to point out that different assumptions about—say—dimensionality or number of candidates might produce different results. Indeed, we would expect it and have tried to design a method of extracting conclusions from a model that is appropriate for the many different models designers might want to study—even if the most appropriate models turn out not to employ the spatial framework. At the same time we have made a considerable effort to incorporate interesting and plausible detail into the cases that we will study below. So while the results we obtain are not general, we believe that there probably are a number of real-world situations similar enough to our model to be illuminated by the results we obtain. We turn now to the details of the model we will investigate.

Size of Electorate. We have chosen the sizes 21 and 1000 because we wish to be able to make some statements about differences due to electorate size (i.e., about the effects of sampling fluctuation). We use 21 because it is a plausible size for voluntary organizations. We use 1000 as a large electorate size since this is a number large enough to yield substantial expected frequency for each of the 24 possible preference orderings in the 4-candidate elections we wish to investigate. We use 1000, in effect, as a poor person's approximation of an infinite electorate, and while some of our results on large electorates will be due to sampling fluctuation, the overwhelming majority should be due to genuine parametric variation. We consider 10 electorates of each size, and this will permit us to place some bounds on the sampling component of our findings.

Dimensionality of Issues. All the electorates studied here have the same dimensionality and the same covariance structure. These are aspects of the electorates that it would certainly be interesting to vary in subsequent work.[10] All voters are represented by their ideal points in a four-dimensional space, that is, by a four-tuple of real numbers. The four numbers are generated in the following way: voter *i*'s position on the first dimension, Y_{1i}, is chosen from a standard Gaussian (normal) distribution (mean zero, standard deviation 1.0). The voter's position on the second dimension Y_{2i}, is generated from the first dimension position by perturbing it with Gaussian noise of mean zero and standard deviation 1.4. The third position is produced from the second with fresh noise of similar character, and the fourth is generated from the third in the same fashion. The values on all dimensions are then normalized to have unit variance. On the average, all the electorates studied here are characterized by the matrix of correlations in Table 3.

The average of the six correlation coefficients is .54. For comparison, Converse (1964) reports a mean interissue correlation of .53 for an elite sample and .23 for a mass sample. Achen (1975) has argued that these are underestimates of the true correlations as a result of instrument vagueness. The standard deviation we have used for the noise variable results in an average correlation that would be appropriate for elites if Converse is correct, and for masses if Achen is.

Differentiation of Candidates. In choosing sets of four candidates that can be combined with electorates to generate elections, we badly need theoretical guidance that is, to our knowledge, not available. A good argument can be made that competition for votes will force candidates to cluster near the center of mass of the voters, in something like the dynamic demonstrated for the two-candidate case, but this has not been proven.[11] On the other hand, one could argue plausibly that, at least in some circumstances, candidates can be characterized as random draws from the population of voters or even as being, on the average, more extreme in their positions than the voters are. The case of candidates who resemble randomly chosen voters would occur if candidate selection is unbiased and is an obvious null model. The case of candidates more extreme than the voters would occur if the probability of becoming a candidate is correlated with extremeness. We have chosen to study each of these three possibilities by establishing three groups of candidate sets, one with much less dispersion than the distribution of voters, one with the same dispersion, and one with substantially greater dispersion. A candidate set always consists of four four-tuples of real numbers, each four-tuple giving the location of a candidate in the same space in which the electorate is positioned. The four-tuple for candidate *j* is produced in a manner parallelling that used to locate voters. A first dimension value is chosen from a Gaussian distribution with mean zero; the subsequent three dimension values are produced by repeated applications of fresh Gaussian noise and the values are then normalized to have unit variance. We have used three different variances. One group of candidate sets is generated with first dimension variance of .04. The second group has variance 1.0, the same as the voters. The third group has variance 1.5. In each group the noise used to generate values for the dimensions is chosen so as to reproduce the covariance structure of the voters. The expected correlations are thus identical to those in Table 3. This method was used to generate 30 sets of candidates under each of the 3 levels of candidate dispersion.

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[10] The only precedent for the use of a spatial model of voter preference to evaluate the performance of social choice functions is Tullock and Campbell (1970). They consider voters distributed uniformly in an issue space, an assumption we drop in favor of the more plausible assumption that voters are normally distributed in the issue space. Their results suggest that the probability of a Condorcet winner decreases slightly as the number of dimensions in their model increases.

[11] See the earlier references to work on the spatial model. Downs (1957, Chs. 8 and 9) discusses the difficulties which arise with more than two candidates.

We have now characterized the procedures used to generate 10 large and 10 small electorates and 3 groups of 30 candidate sets. These can be combined to produce 60 sets of 30 elections each. The elections—sets of frequencies on the possible preference orderings—are generated by a computer program that combines an electorate set and a candidate set under a metric. For a given electorate set and candidate set, the program computes the Euclidean distance from voter *i* to each candidate. From the four distances a preference ordering for voter *i* over the four candidates is determined, the closest candidate being most preferred. Since the distances are real numbers, ties have probability zero.[12] We have then counted the resulting preference ordering for each voter in the electorate and the relative frequencies of the 24 possible orderings of the four candidates constitute a single election. In addition to these 1800 elections, we generated 600 elections (300 each for 21 and 1000 voters) using the impartial culture assumption in order to compare the spatial results with those from the traditional model.

Table 3. Expected Correlations Among Voter Dimensions

Dimension	1	2	3
2	.45	–	–
3	.33	.75	–
4	.28	.68	.83

Results

The substitution of a spatial model for the impartial culture assumption of previous work has major effects on the likelihood that a Condorcet winner will exist in any particular election. As Table 4 shows, the impartial culture assumption results in a serious underestimate of the frequency with which Condorcet winners occur, if naturally occurring preferences are more like those generated by the spatial model. The differences between the proportions for the random preferences and those for the spatial preferences are significant at the .05 level.[13] The only case in which the paradox of voting occurs with important frequency is that of small electorates choosing among candidates who are all quite close to the center of the theoretical population of voters.

Table 4. Proportion of Elections with Condorcet Winners

	Electorate Size	
	21	1000
Impartial Culture	.84	.85
Candidate Dispersion		
Low	.92	.99
Medium	.98	1.00
High	.98	1.00

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[12] Ties in the digital computer representations of the distances, if any occurred, were broken randomly.

[13] The 300 elections for each size-dispersion class are not independent. For each voter set we have aggregated the elections in each of the 30 candidate sets and we have used the aggregate measures, which are independent, in all of our analyses. There are thus 10 aggregate cases for each cell in Table 4 that pertains to the spatial model.

As mentioned earlier, the criterion we use to compare the four social choice functions is their ability to select Condorcet winners when they exist. The fact that under the spatial model Condorcet winners are even more likely to exist than under the assumption of random preferences, reinforces the usefulness of this criterion.[14] Table 5 shows the proportion of Condorcet winners selected under the various conditions. Under all eight conditions, plurality voting is significantly (at the .01 level) worse at selecting Condorcet winners than the other three functions.[15] Among the other functions, the differences between the results under the random and spatial results are striking. Under the traditional impartial culture assumptions there are no significant differences between the abilities of Coombs, Borda, and Hare to select Condorcet winners with 1000 voters, and with 21 voters, only the small Borda-Coombs difference is significant. Under the spatial assumptions, the differences are much greater, and it becomes apparent that the Coombs function is generally superior to the other two in selecting Condorcet winners. With 1000 voters, Coombs is significantly better (with one exception) than Borda, which is significantly better than Hare. The exception to this conclusion is the low-dispersion case in which no significant difference exists between Borda and Coombs. With 21 voters, Coombs does significantly better (at the .01 level) than Borda and Hare, whose performances are significantly different only under low candidate dispersion. Thus if preferences in our hypothetical organization could be adequately characterized by our spatial assumptions, and if the organization were concerned to pick Condorcet winners for its presidency, then it might want to use the Coombs function or possibly the Borda rule. It would certainly want to avoid plurality voting.

Table 6 shows the results of analyses of variance of the spatial data summarized in Table 5.[16] These statistics indicate that parametric variation in the spatial model generally has an extremely important effect on the ability of these social choice functions to select Condorcet winners. Increasing electorate size produces substantial decreases in the Condorcet effectiveness of Hare and Plurality. All four systems become increasingly able to select Condorcet winners as dispersion increases, that is, as the process generating candidate positions produces alternatives that are more "extreme" or tend more often to lie on the "fringes" of the electorate. All three effects are weakest for the Borda rule. This may be due to the fact that the Borda rule makes simultaneous use of all information contained in individual preference orders.

The conclusions drawn from these tables can be summarized as follows:

(1) The differences between the results under a model of random preferences and those under a model of spatial preferences are very significant.

(2) Parametric variation in the spatial model has very significant effects on the ability of the four social choice functions to select Condorcet winners.

Table 5. Proportion of Condorcet Winners Selected

	21 Voters				1000 Voters			
	Impartial Culture	Candidate Dispersion			Impartial Culture	Candidate Dispersion		
		Low	Medium	High		Low	Medium	High
Coombs	.93	.96	.98	.99	.91	.81	.99	.99
Borda	.86	.83	.83	.92	.89	.85	.86	.97
Hare	.92	.72	.75	.90	.92	.32	.60	.84
Plurality	.69	.59	.53	.77	.69	.27	.33	.70

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[14] There are other criteria that could be used to evaluate social choice functions. For instance, Fishburn and Gehrlein (1976) investigate the ability of various two-stage voting systems to select Condorcet and Borda winners. The methodology we employ here is sufficiently general that it could be used with a wide variety of such criteria.

[15] These differences were tested using matched-pair *t* tests. For each voter set, the data were aggregated for the 30 candidate sets, and the difference between the performances of each pair of social choice functions was computed. We then tested the hypothesis that the mean difference was zero.

[16] The data were aggregated as before. There were thus ten data points for each size-dispersion cell.

(3) For the range of cases we have studied, the four social choice functions can be ordered as follows in their overall ability to select Condorcet winners: Coombs, Borda, Hare, Plurality.

Interpretation

The reasons for many of the differences among the results just presented can be derived from the nature of the spatial model. This model generates elections which are nonuniform, and the votes tend to cluster on those preference orders in which candidates near the center of the electorate are ranked high. One strong indication of the nonuniformity of the elections and its effects on the outcomes is given by Table 7, which shows, for the various conditions, the proportion of elections in which some candidate received a majority of first-place votes. It is obvious that the spatial model is much more likely to generate elections with a majority winner than the impartial culture model, even when the electorate is large.

The effects of parametric variation in the spatial model on the ability of the different social choice functions to select Condorcet winners can be illustrated using the one-dimensional case. In particular consider a large electorate with four candidates distributed near the center of the electorate, as in our 1000-voter, low-dispersion case. This situation is shown in Figure 3, in which the candidates have been numbered in order of their plurality vote. Note that the candidates with the largest plurality totals are the extreme candidates. In the one-dimensional case, the candidate located closest to the median voter will always be a Condorcet winner, which in this case is candidate 4, the plurality *loser*. When candidates are tightly clustered near the center of the voter distribution, the plurality social choice function often chooses an extreme candidate. Hare performs similarly, often eliminating the central candidates in the process of generating a runoff between the extremes. The Hare function, by virtue of containing one pairwise majority vote at its final stage, is less likely to select the most extreme candidate, but the next most extreme candidate may frequently be selected. These effects are illustrated by Table 8, which shows, for the case of 1000 voters with low dispersion, the social choices as a function of the distance of the candidates from the center of the voter population (not the sample). This table clearly indicates the tendencies of the plurality and Hare social choice functions. The modal case for plurality voting is to choose the most extreme candidate, while Hare is almost as likely to choose the next most extreme candidate as it is to choose either candidate closer to the center. The spatial model (particularly with low candidate dispersion) feeds the tendencies of these two social choice functions, and leads to the selection of extreme candidates. For the Coombs and Borda social choice functions, the opposite is true. They are much more likely to select central candidates, as is the Condorcet criterion. The Coombs function, by eliminating extreme candidates first, almost always selects one of the two candidates nearest the center of the theoretical voter population.[17] The Borda function accomplishes the same results, although the process is somewhat different. Young (1974) has shown that the alternative it selects is the one with the highest average margin of victory in pairwise comparisons. The Condorcet criterion uses a closely related logic in selecting an alternative that wins a majority in every pairwise comparison, if one exists.

We might summarize by saying that in a case where voter preference orderings are reasonably well described by the spatial model, and candi-

Table 6. F Ratios from Analysis of Variance

	Source		
	Size	Dispersion	Interaction
Coombs	16.2*	45.9*	28.1*
Borda	3.3**	20.7*	.3
Hare	41.1*	119.4*	29.2*
Plurality	36.7*	97.9*	14.3*

*Significant at .01 level
**Significant at .10 level

Table 7. Proportion of Elections with Majority Winners

	Electorate Size	
	21	1000
Impartial Culture	.03	.00
Candidate Dispersion		
Low	.30	.00
Medium	.10	.10
High	.40	.30

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[17] Indeed, Coombs (1954, pp. 397–99) has shown that if the issue space is one-dimensional and if preferences are singlepeaked, then his function always selects the Condorcet winner.

date positions have the central tendency usually associated with the Downsian tradition, the Plurality and Hare systems will frequently pick the more extreme alternatives while Coombs, Borda and the Condorcet criterion will favor those near the center of the voter population. This observation holds strictly only for the four-dimension, four-candidate case under the spatial model as we have simulated it, but we think it likely that similar results hold for other numbers of dimensions and candidates and our methods can be used to investigate the conjecture.

A Digression on Primary Elections

Our discussion thus far has focused primarily on the abilities of social choice functions to select Condorcet winners. Before concluding, we wish to discuss another electoral situation in which the framework of analysis we have introduced above could prove useful to political designers studying other properties of social choice functions. The most prominent multicandidate elections in the U.S. are the quadrennial presidential primaries leading up to the nominating conventions. The objectives of these elections include both the selection of a winning candidate and the estimation of the relative strengths of all of the candidates based on their shares of the vote. These vote shares are frequently used to apportion convention delegates, but they also serve as the raw material for political observers' evaluations of the candidates' performances relative to expectations. These evaluations in turn influence subsequent expectations and the supply of political resources in future primaries.

Of the four social choice functions we have studied above, only plurality voting and the Borda rule yield measures of vote shares appropriate to the objectives of a primary election, and we will therefore confine our analysis to these two functions. It should be noted that the Borda rule seems particularly appropriate for use in primary elections, because it orders candidates by the average ranks assigned to them by the voters, a measure which is presumably closely related to the relative strengths of the candidates. We have used the elections generated from the spatial and impartial culture models to simulate primary elections involving four candidates. Table 9 shows, for the cases involving 1000 voters, four measures of the differences between the results of primary elections using plurality voting and the Borda rule. These measures are the proportion of elections in which the two functions yield the same ordering of the candidates, the proportion in which the top two candidates are the same, the proportion in which the winning candidate is the same, and finally an overall measure of agreement, the proportion of the time the two functions agree on the rank they assign to any

Table 8. Social Choices and Distance from Center of Electorate

	Nearness to Center of the Theoretical Electorate			
	Nearest			Furthest
Condorcet	.87	.11	.02	.00
Coombs	.75	.20	.05	.00
Borda	.81	.17	.02	.00
Hare	.33	.33	.29	.05
Plurality	.23	.27	.12	.38

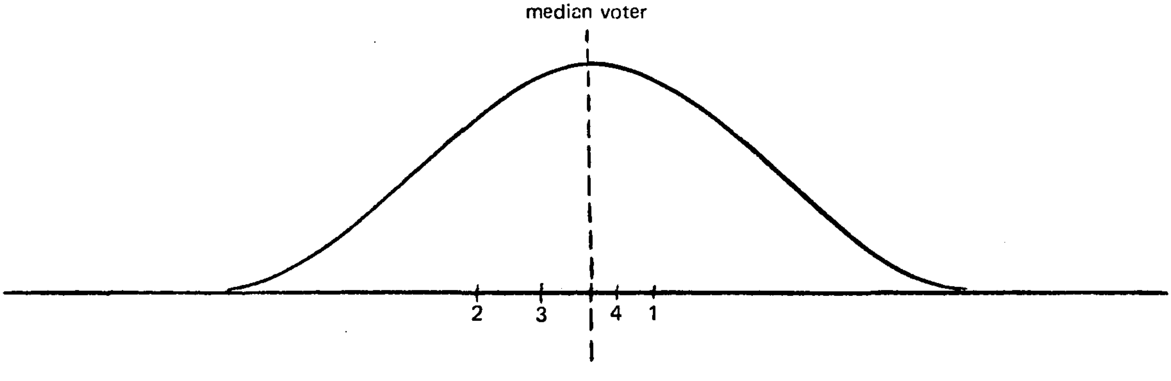

Figure 3. One-Dimensional Example

Table 9. Measures of Agreement Between Plurality and Borda Social Choice Functions

Measure	Impartial Culture	Dispersion Low	Dispersion Medium	Dispersion High
Same Order	.26	.11	.12	.10
Same Top Two	.39	.13	.14	.13
Same Winner	.71	.26	.39	.38
Same Rank	.54	.35	.38	.42

given candidate.[18] There are no significant differences among the spatial model results for the first two measures, and for each measure all spatial model results are significantly different from those of the random case at the .05 level. For the third measure, selecting the same winner, only the medium/high difference is not significant at the .05 level. For the final measure, all differences are significant at the .05 level. These results make very clear the important effects upon primary election outcomes of the selection of a social choice function. They can be roughly summarized as implying that in primaries corresponding well to the case we have studied a given candidate would be ranked similarly by the two social functions only about 40 percent of the time. Our earlier results showed that where they differ, Borda would favor the more central candidates, plurality the more extreme. The question of whether centrality is to be valued in American presidential primaries we leave open.

Even in those cases where plurality voting and the Borda rule yield the same ordering of the candidates, there can be important differences in the political effects of the results using the two functions. This can be illustrated using data from the 1970 SRC Election Study on four prominent candidates for the Democratic presidential nomination (Muskie, Kennedy, Lindsay, and Humphrey). The "thermometer ratings" assigned to these candidates by the survey respondents were used to construct preference orderings, which were then used to simulate a national primary with these four candidates. The results for the plurality and Borda social choice functions are shown in Table 10. Both functions yield the same order on the candidates, but there remain important differences between the vote shares under the two functions. Not only would they apportion convention delegates differently in systems that are not winner-take-all, but it is likely that most observers would offer quite different interpretations of these sets of vote shares. The plurality results suggest a neck-and-neck race between Muskie and Kennedy, followed at a fair distance by Lindsay, with Humphrey a distant fourth. The Borda results suggest a much clearer victory for Muskie (due to a large number of second-place votes, as was widely believed to be true in his case in 1970), with the other three candidates grouped within two percentage points of each other. Thus even in a case in which both social choice functions agree not only on the winner, but on the entire ordering, there may still be extremely important political information which is reported quite differently by the two functions.

The differences between the plurality and Borda vote shares for the elections generated by the spatial and the impartial culture models attest to the sensitivity of the vote shares to both the selection of a social choice function and the distribution of preferences in an electoral situation. Table 11 shows the median absolute percentage difference between plurality and Borda vote shares. In the elections

Table 10. Results of Simulated National Primary

Candidates	Plurality Votes	Plurality %	Borda Rule Points	Borda Rule %
Muskie	340	28.7	2029	28.5
Kennedy	339	28.6	1771	24.9
Lindsay	273	23.0	1678	23.6
Humphrey	233	19.7	1632	22.9

Table 11. Median Absolute Differences Between Vote Shares (Percent)

Candidate Set	Median Value
Impartial Culture	.67
Low	9.58
Medium	8.13
High	8.93

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[18] Statistical analyses were carried out as described earlier.

derived from the spatial model, these differences are substantial. A candidate's vote shares from the two functions are as likely to differ by more than nine percentage points as they are to differ by less. These results, combined with those in Table 9, strongly suggest that primary elections using plurality voting and the Borda rule are very likely to yield substantially different outcomes if preferences are similar to those generated by the spatial model. For the elections generated from the impartial culture model, the differences are very small, reflecting the fact that these elections are nearly uniform when there are 1000 voters.

Part of the explanation for the large differences in the spatial model cases is the fact that the variance of the plurality vote shares is at least four times larger than the variance of the Borda vote shares. An additional implication of this difference for primaries is that Borda is less likely to generate clear winners or losers, because the vote shares with this function are clustered much closer together than those for plurality voting.

Conclusion

We have now given a number of examples of questions of potential interest to political designers that can be explored within a framework of an election space and a model that generates the probabilities associated with regions of that space that correspond to events of interest: competitiveness of congressional seats, differences in primary election winners and in percentages attained in primaries by particular candidates, and, of course, our extended investigation of the selection of Condorcet winners by four social choice functions. These illustrations are from a large class of cases in which definite estimates of likelihood can be obtained for events of significance. The quality of those estimates clearly depends on the quality of the model of the electoral situation used to generate the underlying probabilities on election space. That generation may be by analytic solution of a probabilistic model—which would probably be preferable but appears quite difficult—or by Monte Carlo methods such as those we have employed here which draw points from the density on election space rather than explicitly specifying and integrating it.[19]

However the probabilities are generated, we would argue that to be genuinely useful such estimates must flow from a model that seems to take into account the features of the design context that may significantly affect patterns of preferences. In the development of our main example we studied a number of such features: size of the electorate, the number and interdependence of dimensions in preference space, the number of candidates and the dispersion of the process selecting them. This choice of features seems reasonably natural to us, but hardly inevitable. In particular, many of them are suggested by the spatial conception of preference generation on which we have relied. The spatial model may or may not turn out to be a good approximation of the process that generates elections. Whether it does or not, political designers will want to investigate assumptions quite different from those we have studied.

Nonetheless, the results we have obtained demonstrate the sharp sensitivity of social choice function performance to the character of the process that generates elections. They therefore establish the necessity of taking features of the electoral situation into account in the application of social choice theory. The method of defining events of interest to political designers as regions of election space and assessing their probabilities appears to us to permit the recasting of substantial portions of social choice theory into the applicable form that seems to be required.

Appendix

The problem we have analyzed above may be formally represented in the following way: Let $S = \{s_j \mid j=1, 2, \ldots, n\}$ be a finite set of alternatives (candidates, proposals, social states, etc.) from which a social choice is to be made, and let $\Pi(S) = \{\pi_i \mid i=1, 2, \ldots, n!\}$ be the set of permutations of the elements of S, in this case representing the $n!$ linear preference orders over S. An election is defined to be a relative frequency distribution on the elements of $\Pi(S)$. The set of all possible elections is

$$X = \{\bar{x}=(x_1, x_2, \ldots, x_{n!}) \mid x_i \geq 0, \sum_{i=1}^{n!} x_i = 1\},$$

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[19] A third method we are exploring is to draw points from an explicitly specified density function. An example might be the Dirichlet density on the $k!$ dimensional simplex with parameters derived from a theoretical model of the underlying political process.

where x_i is the relative frequency of preference order π_i. A social choice function is a mapping from X onto the set of nonempty subsets of S. That is, a social choice function selects a winning alternative (or set of alternatives if no unique choice can be made). A social decision function is a mapping from X onto the set of nonempty subsets of $\Pi(S)$. A social decision function, such as the primary elections discussed above, selects a winning ordering (or orderings if no unique choice can be made).

The first social choice function we consider is *plurality* voting, in which each voter casts a vote for his/her most preferred candidate, and the candidate with the most votes is elected. Plurality voting has the dubious distinction of being both the social choice function which is most widely used in this country and the function whose axiomatic properties are least satisfactory among those commonly discussed in the social choice literature (Richelson, 1975). Formally, if $P(\bar{x})$ is the plurality choice set for election $\bar{x}$, then

$$P(\bar{x}) = \{s_j \epsilon S \mid \sum_{i \epsilon I_j(S)} x_i \geqslant \sum_{i \epsilon I_k(S)} x_i,\ \forall s_k \epsilon S\}$$

where $I_j(S) = \{i \mid s_j$ is most preferred among the elements of S in $\pi_i\}$. When used as a social decision function, plurality voting yields a social decision set

$$P'(\bar{x}) = \{\pi_i \epsilon \Pi(S) \mid s_j \text{ is ranked above } s_k \text{ in } \pi_i$$

$$\text{only if } \sum_{i \epsilon I_j(S)} x_i \geqslant \sum_{i \epsilon I_k(S)} x_i, \text{ for all pairs}$$

$$(s_j, s_k)\}.$$

That is, the plurality social decision orders the candidates according to the number of first place votes each receives.

The second social choice function is the *Hare* (or transferable vote) system, which has long been considered to be a reasonable way around the "wasted vote" problem encountered in plurality voting.[20] Under the Hare system, the candidate with the fewest first-place votes is eliminated and each vote cast for that candidate is transferred to the next ranking candidate on that voter's ballot. This process is continued until some candidate is assigned a majority of the first-place votes. If we indicate the increasingly restricted set of alternatives by superscripting S and denote the alternatives eliminated at the m^{th} stage by $E(\bar{x}, S^{m-1})$, then we can define $S^0 = S$, and for $m \geqslant 1$ $S^m = S^{m-1} - E(x, S^{m-1})$, where $E(\bar{x}, S^{m-1}) =$

$$\{s_j \epsilon S^{m-1} \mid \sum_{i \epsilon I_j(S^{m-1})} x_i \leqslant \sum_{i \epsilon I_k(S^{m-1})} x_i,$$

$$\forall s_k \epsilon S^{m-1}\}.$$

The choice set of the Hare social choice function is $H(\bar{x}) = S^{n-1}$.

The third social choice function is the *Coombs* system, which is similar in spirit to the Hare system, but uses a different elimination logic (Coombs, 1954, pp. 397–99). Under the Coombs system, the candidate with the most last-place votes (the most disliked candidate, as opposed to the least liked candidate under the Hare system) is eliminated, and a first-place vote cast for that candidate is transferred to the next-ranking candidate on that voter's ballot. This process is continued until some candidate is assigned a majority of the votes. Here we use $I_j'(S)$ to denote the preference orders π_i in which s_j is ranked last among the alternatives in S.

Once again, let $S^0 = S$ and let S^m be the set of candidates remaining at the m^{th} stage of the process. Let

$$Q(\bar{x}, S^m) = \{s_j \epsilon S^m \mid \sum_{i \epsilon I_j(S^m)} x_i \geqslant \tfrac{1}{2}\}$$

be the set of majority winners at stage m and let the set of candidates deleted at stage m be

$$D(\bar{x}, S^{m-1}) = \{s_j \epsilon S^{m-1} \mid \sum_{i \epsilon I'_j(S^{m-1})} x_i \geqslant$$

$$\sum_{i \epsilon I'_k(S^{m-1})} x_i,\ \forall s_k \epsilon S^{m-1}\}.$$

The set S^m is then given by

$$S^m = \begin{cases} Q(\bar{x}, S^{m-1}), \text{ if } Q(\bar{x}, S^{m-1}) \neq \phi \\ S^{m-1} - D(\bar{x}, S^{m-1}), \text{ otherwise.} \end{cases}$$

The choice set for the Coombs social choice function is $C(\bar{x}) = S^{n-1}$.

The final social choice function is the *Borda* rule, which is radically different from the other three. It is the only system considered here which takes into account each voter's complete preference ordering in a single step in arriving at a social choice and its axiomatic foundation is impressive (Young, 1974). Formally, the Borda choice set is

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[20] An excellent discussion of this social choice function can be found in Black (1958). Doron and Kronick (1977) discuss one logical problem with this function.

$$B(\bar{x}) = \{s_j \epsilon S \mid \sum_{i=1}^{n!} r_i(s_j) \cdot x_i \geqslant \sum_{i=1}^{n!} r_i(s_k) \cdot x_i,$$

$$\forall s_k \epsilon S\},$$

where $r_i(s_j)$ = the number of alternatives to which s_j is preferred in π_i. Under this "method of marks" an alternative receives $(n-1)$ points for each time it is ranked first, $(n-2)$ points for each time it is ranked second, etc. The winning alternative is that with the highest average rank. When used as a social decision function, the Borda social decision set is

$$B'(\bar{x}) = \{\pi_i \epsilon \Pi(S) \mid s_j \text{ is ranked above } s_k \text{ in } \pi_i$$

$$\text{only if } \sum_{i=1}^{n!} r_i(s_j) \cdot x_i \geqslant \sum_{i=1}^{n!} r_i(s_k) \cdot x_i, \text{ for all}$$

$$\text{pairs } (s_j, s_k)\}.$$

The Borda social decision function orders candidates according to their average ranks.

Assessing how often events of interest occur under various social choice functions requires that they be evaluated with respect to a set of elections which reflect the empirical realities of a given "electoral situation." Let $F_E(X)$ be the probability density of elections for the electoral situation E. Factors influencing $F_E(X)$ would include, among others, the size of the electorate, constitutional constraints, communication patterns among the electorate, the nomination process, and demographic processes. Examples of electoral situations are elections to the U.S. House of Representatives, presidential primaries, elections to the executive committee of an academic department, the choice of the foreperson of a jury, etc. If we denote by $G(X) \subset X$ the set of elections for which some particular event occurs (e.g., $G(X) = \{\bar{x} \epsilon X \mid P(\bar{x}) \neq B(\bar{x})\}$), then the probability of this event in electoral situation E is given by the value of the integral $\int_{G(X)} F_E(X)$.

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COMMUNICATIONS

The J-Curve Theory

To the Editor:

In 1962 I published a general but not universal theory of revolution (Davies, 1962). Calling it the J-curve, I proposed that when a long period of rising expectations and gratifications is followed by a short period during which expectations continue to rise while gratifications fall off sharply, the probability of civil violence against government rises rapidly. A gap that widens quickly makes revolution likely. The theory has been criticized and transmogrified into first a V-curve (Grofman and Muller, 1973) and then an M-curve (Miller et al., 1977). Lest we begin to observe the Yo-Yo Effect, some clarifications are clearly needed.

The theory is psychologically based in the basic needs of human beings. The most elemental of these needs are the physical, material needs for food, clothing, shelter, health, and physical safety. Others are for association (the social-affectional needs), equality and dignity (or self-esteem), and self-actualization. The set of basic needs that makes most sense to me[1] is derived from Abraham Maslow's 1943 list. I did not spell these needs out so clearly in the 1962 version of the theory as in the 1969 version (Davies, 1969), but in both instances, as well as in *Human Nature in Politics* (New York: Wiley, 1977, Ch. 1) and in the "Priority of Human Needs . . ." paper (Davies, 1977), I tried to emphasize that human beings want material goods but much more. Unfortunately, not just economists but also political scientists interested in relating human needs to political behavior have been impeded in their efforts to test such relationships by the fact that the most abundant data are economic and they relate, when considered by themselves, only to one of several basic human needs.

In the 1969 statement of the theory, I applied an idea generated originally about revolution to a rebellion that fell short of seizure of the state. The Black Rebellion of the 1960s, not very surprisingly, seemed to fit the J-curve. In referring to the course of black development since emancipation in 1863, I said that until the 1940s black people were overwhelmingly concerned simply with survival—with satisfying their physical needs. Developments during World War II, including the beginning of integration in the armed forces and more equal job opportunities in war industry, were economic but something else, too. I said: "These advances relate to jobs and housing and therefore to the physical needs, but they also—notably in the case of sport participation—have overtones of equal dignity" (Davies, 1969, p. 720).

The one systematic statistical measure I was able to use was a comparison of income with years of education achieved among black people, for the years 1940–1967.[2] This comparison, expressed as a ratio between income and education, measured the gap not just between material expectations and gratifications but also the gap between expectations and gratifications pertaining to equality of opportunity. In the early 1950s, the gap began to widen, as the increased number of years of schooling among blacks was not paralleled by increased income.

In addition to this gap, involving both economic and equality needs, I mentioned others, most particularly the gap relating to increased violence of whites against blacks, following efforts to integrate schools, parks, lunch counters, and buses. This violence reactivated the need for physical safety. The gaps developed in the mid-1950s and early 1960s. I placed the peak—following which the total gap began to widen rapidly—in the pitched battle of April 1973, when police in Birmingham, Alabama turned themselves, white people, fire hoses, and dogs on black people. The first major concerted black violence I deemed to be the Watts riots in Los Angeles in August 1965, thus taking that date as the commencement of the Black Rebellion.

In a sedulous effort to test the J-curve in that rebellion, three people have recently concluded that, as judged by their data, "in the case of each of the four hypotheses relating the J-curve to the black urban riots of the 1960s, the theory was rejected" (Miller et al., 1977, p. 978). Each of the hypotheses related to only one of the physical needs: what the authors described as "financial" expectations, derived from data of the University of Michigan Survey Research Center, for the years 1956–1968. All these hypotheses pertain only to the economic aspect of the physical needs and include neither the violence which threatened physical need deprivation of another sort, nor the declining satisfaction of the needs for equality and dignity.

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[1] In a revision of my first use of it in 1963—dividing needs into substantive and instrumental and including security, knowledge, and power in the latter category. (See Davies, 1977.)

[2] This measure was developed by Harmon Zeigler and Jerry B. Jenkins out of U.S. Census Bureau data.

The appraisal by Miller, Bolce, and Halligan is thus radically deficient as a test of the J-curve, both on theoretical grounds and as an empirical test. The authors give evidence of having read what I wrote in 1969 about other than economic needs of black people, but they do not say why they suppose the causes of the rebellious behavior of black people can be analyzed as purely economic. Indeed they seem to be unaware of, or at least to ignore, the fact that the statistical measure I used relating black income to education was not simply an economic measure. Like conservative economists and conservative Marxists, they seem to suppose, dismally, that human beings including the black variation have only material desires or that nonmaterial desires have no effect on their political behavior. They forget the equalitarian demands of 1776 and 1789 that became national summonses of the American and French Revolutions. In short, their theoretical attack on the J-curve is both psychologically and historically innocent.

It is discouraging but not surprising to note psychological innocence among social scientists, but it is both discouraging *and* surprising to note quantitative naivete. I described the last steady rise in black expectations as starting about 1940, and with some fluctuations in the late 1950s and early 1960s ending with a peak in 1963 in Birmingham and with the beginning of the Black Rebellion two years later in Los Angeles. The fluctuations—as the gap between income and education indicated—appeared from 1957 to 1965. It is during this very period that Miller et al. used data indicating some fluctuations in "financial" expectations that form a kind of "M." They refute an application of the theory to a time span of 1940–1967 by analysis of data gathered for the years 1956–1968. The J-curve is myopically converted into a Big M, with no prior buildup in the minds of blacks—with no memory of developments before the Survey Research Center gathered the data.

Miller, Bolce, and Halligan ignore the psychological base of the theory in human motivation and test the theory on only half the cycle I described. They read a bit like the people who say, in the eye of the hurricane: it is obvious that any prediction of a storm is clearly rejected by the blue sky overhead. Or people who say the cause of the storm is its own fluctuations. Social science is capable of more thoughtful tests than that of a basic theory of civil violence.

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TO THE EDITOR:

In our article (Miller, Bolce and Halligan, 1977) we addressed several aspects of the J-Curve theory that appeared to be both conceptually and empirically questionable. One of our concerns was the issue which James C. Davies now underscores in his communication, i.e., the theory is psychologically based. Unfortunately, neither in his two previous renditions of the theory (Davies, 1962, 1969) nor in the present communication does he deal with this issue as it applies to the testing of the theory. We agree that the J-Curve theory is psychologically based. We also agree that a person's positive appraisal of his or her economic situation is central to a sense of psychological well-being. We argue now, as we did then, that our use of individual-level data of black and white perceptions of economic expectations and satisfactions is the most appropriate psychological test of the J-Curve theory to date.

In contrast to our rigorous empirical testing of the theory, we hasten to confront Davies with the inadequacy of his own test of the theory. As he notes, "the one systematic statistical measure I was able to use was a comparison of income with years of education achieved among black people, for the years 1940–1967." Because the theory is rooted in the psychological needs and expectations of individuals, a non-psychological aggregate measure is hardly an appropriate operationalization for assessing the theory. Not only is the important level of analysis question ignored, if not violated, but also, aside from Davies' unsubstantiated assertions, there is not one iota of evidence to suggest that Davies' measure assesses a form of relative deprivation that was part of the psychological experience of the black community. Moreover, embellishing this rather inappropriate measure, almost a decade later, with the belated afterthought that it also possessed the prowess to measure equality of opportunity only further adds to its lack of credibility.

Consider further that the graphic representation of this measure, as Davies acknowledges, puts the riots almost a decade too soon. Since the constraints of the data were not properly acknowledged, Davies moved the intolerable gap forward almost a decade. When we replicated Davies' operationalization, using the individual-level data, we too found that the J-Curve pattern did not emerge. Feeling constrained by the empirical consequences of our results, we, however, could not produce a curve based on our interpretation and intuition of how the black population perceived events.

We did not set out in the course of our research to either "confirm" or reject the theory, nor were we interested in transforming—or "transmogrifying" as Davies put it—the shape of the J-Curve. We were interested in testing the theory against the best empirical evidence available. Grofman and Muller (1973) also found that the theory does not withstand the rigors of empirical testing, perhaps serious consideration should be given to a reformulation of the theory and to making its empirical attributes more precise.

In this latter regard, let us reexamine the theory. The theory proposes that when a long period of rising expectations and gratifications is followed by a short period in which gratifications fall off sharply and create an intolerable gap between actual gratifications and continually rising expectations, the probability of civil violence increases rapidly. When one is confronted with the problem of assessing this theory empirically, several questions come to the fore. How long is a long period? How short is a short period? How sharply must gratifications fall? How large is an intolerable gap? In other words, how will we know a J-Curve when we see it?

These are not easy questions to answer. The theory itself is vague and gives no operational guidance. Consider how an analogous theoretical statement in the natural sciences might appear. The proposition "Water solidifies when sufficiently cooled" is at about the level of specificity of the J-Curve.

Given the imprecision of the theory and the fact that it is in ex post facto statement—the revolution always precedes the theory—it is possible to fit a J-Curve to any revolutionary situation. Since the theory gives no direction as to what social, economic or political indicators are to be selected, leaving the researcher to choose any indicator, from any realm, for any data, point, or to skip a data point or two or three (or however many one pleases), one can construct a J-Curve, or any other curve, for that matter. One is even free to ignore certain indicators. Davies, for example, ignored the civil rights acts of 1964 and 1965, and was untroubled by the fact that the long period of rising expectations for the urban riots is 12 years (1940–52) while the short period of declining gratifications is 13 years (1952–65).

It is to be recalled that to avoid some of these problems we tested Davies' theory against his own operationalization, albeit at the more appropriate individual level of analysis. Moreover, not only was the theory rejected in this test, but it was also rejected in a series of psychological measures directly derived from the theory. In performing these tests, we did not sketch curves based on some intuition of the quantitative amplitudes of selected qualitative events. When such methodology is devised, we would appreciate learning how to implement it. If this constitutes historical and quantitative naivete, we wear those badges proudly. If data, appropriate methodology, and precise theoretical formulations can be adduced to inspire greater confidence in the J-Curve, we would welcome them. Perhaps, Davies will turn his considerable talents toward such endeavors. Until that time, we stand by our empirical results. The J-Curve theory does not explain the black urban riots, and Davies has not presented then or now one useful shred of evidence to make us think otherwise.

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Trivial Shifts in Presidential Elections

TO THE EDITOR:

Steven J. Rosenstone and Raymond E. Wolfinger (1978) have supplied a comprehensive review of interstate variation in voter registration laws and an insightful, well-written analysis of the political consequences likely to follow the adoption of uniformly permissive registration procedures in all states (that is, in effect, national election-day registration).

In their otherwise masterful presentation, they have, I believe, understated the political consequences which national election-day registration might have in close presidential elections. Determining that the most permissive registration procedures applied nationwide would have expanded the 1972 electorate by 12.2 million voters, Rosenstone and Wolfinger found that the expansion of the electorate would have decreased the Republican share of the electorate from 36.4 percent to 35.9 percent; the authors regard this change as "wholly insignificant."[1] The Democratic proportion of the electorate would have increased, as a result of reform, from 51.1 percent to 51.4 percent, a shift they call "equally trivial" (1978, p. 39).

However, the fact that three of the last five presidential elections have yielded an almost even division of popular votes between Democratic and Republican candidates—coupled with the idiosyncracies of the electoral vote system—suggests that even small changes in the electorate following registration reform could take on considerable significance. The swing of 0.8 percent in favor of the Democrats (the projected effect of election reform in 1972) exceeds the margin of Kennedy's victory over Nixon in 1960 and roughly matches Nixon's margin over Humphrey in 1968. To view the matter in another way, Rosenstone and Wolfinger's projection that election reform would have added 12.2 million voters to the 1972 electorate breaks down into more than 6.5 million Democratic identifiers, about 3.9 million Republicans, and 1.7 million Independents.[2] In short, reform would have produced a net increase of Democrats over Republicans of about 2.6 million voters. Recalling that Carter edged Ford by only 1.7 million votes in 1976, one finds it difficult to regard an increased Democratic advantage of 2.6 million voters as either "wholly insignificant" or "trivial." To be sure, one cannot assume that election reform would have produced a net gain of 2.6 million Democratic *votes*. Examination of the voting patterns of Democrats, Republicans and Independents in 1976 does indicate that the projected effects of election reform (applied to 1976) would have added about 630,000 to 900,000 votes to Carter's total.[3] In order to appreciate that the projected effects of election reform could be important, it is worthwhile to recall that a switch to Ford by less than 10,000 Carter votes in Ohio and Hawaii would have elected Ford.

Were a close presidential election like that of 1976 to follow national election reform, it would be impossible to ascertain that the reform had provided the winning margin for the Democratic candidate. Marginal victories, after all, may turn on any number of factors ranging from the caliber of vice-presidential candidates to blunders on televised debates. But the analysis of Rosenstone and Wolfinger tells the Republican leadership that the adoption of election-day registration could very well give the Democratic party a crucial, *almost assured* bonus; correctly, most GOP leaders fear and oppose such election reform. The current and widely publicized effort by the Republican National Committee to court black voters provides a perspective on the issue. The black vote for Carter exceeded his winning margin in eleven states, including Ohio.[4] If the Republican presidential candidate in 1980 were to increase Ford's black vote in 1976 (about 600,000 votes) by 50 to 75 percent, the benefit to the GOP could be entirely offset by the loss resulting from election-day registration.[5] Surely, the potential impact of election-day registration, so ably assessed by Rosenstone and Wolfinger, cannot be regarded as trivial.

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[1] The figures on Republican identifiers include Independents leaning Republican as well as strong and weak Republicans. The Democratic figures are likewise reported.

[2] These are my calculations based on the estimated total potential electorate of 134 million persons in 1972 and the data on the actual and projected partisan composition of the electorate given by Rosenstone and Wolfinger.

[3] This is a rough estimate based on the voting patterns of Democratic, Republican, and Independent identifiers in 1976 applied to the 1972 increment of 12.2 million voters. The lower estimate relies upon the 1976 voting patterns reported by the Survey Research Center of the University of Michigan (Dye and Zeigler, 1978, p. 202). The lower figure also adjusts for the tendency of surveys to overestimate actual presidential vote. The higher estimate is based on *New York Times* survey data supplied in Gerald M. Pomper et al. (1977, p. 61).

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TO THE EDITOR:

Timothy O'Rourke does not quarrel with our estimates of the consequences of election-day registration, but only with our verbal interpretation of the result—a gain of 0.279 percent in the proportion of Democratic identifiers. (For ease of presentation, in our article we rounded this to 0.3 percent.) In other words, the disagreement is over the interpretation of three-tenths of a percentage point.

When based on a survey sample, three-tenths of a percentage point cannot be distinguished statistically from zero. Our hypothetical expanded electorate consisted of 2,044 cases in the University of Michigan Center for Political Studies sample; a difference of .279 percent reflects just six cases. It would be foolhardy to base any predictions about the real world on a shift of six respondents. The sampling error alone is many times greater than .279 percent.

This difference is substantively as well as statistically insignificant. Contrasted to the common belief that election-day registration would produce a Democratic bonanza, a .279 percent gain for the Democrats is indeed trivial.

What of O'Rourke's claim that this difference exceeds the margin of victory in several recent presidential races? To gauge the impact of an expanded electorate, one must look not at the meaningless national popular vote, but at individual state results. The available data do not permit simulation of each state's electorate. However, given the insignificant differences between the actual and expanded national electorates, plus the fact that state-by-state margins of victory are greater than those at the national level, there is no great need for such simulations.

This point can be illustrated if we accept for the moment that the tiny changes in the Michigan sample will occur in the real world and that in each state the expanded electorate will be .279 percent more Democratic and .200 percent more pure Independent. Furthermore, let us assume that in any given election, 80 percent of the Democrats and 50 percent of the Independents will vote for the Democratic presidential candidate. On average, then, .323 percentage points will be added to the Democratic vote in each state (.8 x .279 + .5 x .200).

In recent elections, how many states that voted Republican for president would have gone Democratic if .323 percentage points were taken from the Republicans and added to the Democratic vote total? Of the 200 state contests between 1964 and 1976, only one (in 1976) would have turned out differently. Between 1948 and 1960, only three of 194 state decisions would have changed (one each in 1952, 1956, and 1960). *In no election would the national outcome have been different.*

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[4] The estimate on the contribution of the black vote to the Carter victory comes from an analysis by the Joint Center for Political Studies cited in Hucker (1978).

[5] It should be noted that election-day registration would probably have increased Nixon's margin of victory in 1972. In 1960, 1964, 1968, and 1976, reform would have added Democratic votes. I am inclined to believe that registration reform would benefit any mainstream Democratic candidate in future presidential elections except under highly unusual circumstances.

None of this is to deny that a presidential election *could* be decided by a shift of .323 percent; we only assert that it is an improbable event. The more fundamental point, however, is that three-tenths of a percentage point is statistically equivalent to zero and does not represent an "*almost assured* bonus" for the Democrats. It is more likely an almost assured bonus for no one.

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Voegelin on Gnosticism

TO THE EDITOR:

In his article "Eric Voegelin's Framework for Political Evaluation in His Recently Published Work," Dante Germino (1978) expresses puzzlement over Voegelin's suggestion that the modern gnostics "are spiritually diseased as persons" (this is Germino's paraphrase). Later Germino says that Voegelin presents the problem of reconciling "the demands for intellectual perspicacity with those of human charity."

There is a clear reason for the acerbity of the judgment which Voegelin passes on modern gnosticism, and it is related to the precise nature of his departure from the Lutheran faith. His theology is informed by a sense of sin, and in particular of humanity's capacity for rebellion against God and the natural order which He created. Voegelin says that the modern gnostic engages in rebellion of this kind in his attempt to secure the course of history by recreating human nature.

But as Voegelin shows in his chapter on St. Paul in the fourth volume of *Order and History* (one of the two books on which Germino's article principally concentrates), he finds little significance for his own theology in the passion, death, and resurrection of Christ. Orthodox Protestantism sees these events not only as evidence that Christ was God Incarnate and that we should follow His precepts and His example; it sees them also as symbols of God's infinite mercy, for it believes that, by submitting Himself to suffering and death, Christ made full propitiation for human sins. Having experienced mercy at the hand of God, human beings are thus motivated to be charitable in dealing with their fellow creatures, even when they reproach them.

It remains true that Voegelin is factually correct in ascribing spiritual disorder or, if one prefers, sin, to the modern gnostics. It is also true that *The New Science of Politics* and the chapter on Plato's *Republic* in the third volume of *Order and History* are remarkably prescient accounts of what is happening to this society.

Whether Voegelin's theology is an adequate guide to religious experience is another matter, and one whose significance extends beyond personal salvation. His "framework for political evaluation" provides scarcely any apparatus for choosing one form of nontotalitarian government over another. That many of the critics of liberal democracy are engaged in sinful rebellion he tells us, but that it may have something to commend it, other than its not being totalitarian, he does not say, and cannot say. He cannot say it because he sees correctly that democracy is rooted in part in the gnostic aspirations of men such as Milton, Locke, and John Stuart Mill, and because he will not embrace a Christian framework, whether Protestant or neo-Thomist, which would enable him to affirm it nonetheless.

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Resource Allocations in the 1976 Campaign

TO THE EDITOR:

There has been considerable discussion in the pages of this *Review* and elsewhere on the manner in which the unit-rule feature of the electoral college affects the resource-allocation decisions of candidates in presidential elections. Morton Davis and I, for example, found a substantial large-state bias in the campaign behavior of presidential and vice-presidential candidates in the 1960 through 1972 campaigns based on the number of campaign appearances the candidates made in each state (Brams and Davis, 1974). Analyzing the same data, Colantoni, Levesque, and Ordeshook (1975a) confirmed this bias but offered a different explanation for it, which gave rise to some controversy (Brams and Davis, 1975; Colantoni, Levesque, and Ordeshook, 1975b). Young (1978), who proposed still a different model that predicts allocations directly proportional to the electoral votes of each state, analyzed broadcast expenditure data and similarly found that the candidates spent disproportionally greater amounts on advertising in the large states, but not to the extent predicted by the Brams-Davis "3/2's rule."

Although not originally based on resource-allocation models, the Shapley-Shubik and Banzhaf a priori power indices for *citizens* indicate that those living in the largest states have approximately three times as much voting power as those living in the smallest states (Owen, 1975a, 1975b; see Lake, 1978, for a resource-allocation model that yields the Banzhaf index). (These figures have also generated controversy; see the exchange between Spatt and Owen [1976].) Coincidentally, the 3/2's rule shows up an almost identical disparity between the attractiveness as campaign targets of citizens living in the largest and smallest states.

How well did these models hold up in the 1976 presidential campaign? Data on "campaign stops" of presidential and vice-presidential candidates in the 1976 campaign are now available for comparison with the theoretical predictions of the different models (U.S. Senate, 1977, p. 29). The product-moment correlation coefficients between the number of campaign stops of the 1976 Democratic and Republican slates (presidential and vice-presidential stops combined in each state) and the various model predictions are shown in Table 1. (The Colantoni-Levesque-Ordeshook model makes no single set of predictions and is therefore not included in Table 1.)

It is perhaps surprising that the coefficients are all high and virtually indistinguishable. To be sure, one would expect that the 3/2's rule and the two power indices, which all predict large-state biases and are highly intercorrelated (3/2's rule and Banzhaf index: 0.9996; 3/2's rule and Shapley-Shubik index: 0.9995; Banzhaf index and Shapley-Shubik index: 1.0000), would give very similar results. The proportional rule, however, not only does well, too, but also correlates highly with the 3/2's rule (0.96), the Banzhaf index (0.95), and the Shapley-Shubik index (0.95).

Table 1. Correlation Coefficients for Different Model Predictions and Campaign Stops of Party Slates

	Democratic Slate	Republican Slate
Proportional rule	0.91	0.92
3/2's rule	0.91	0.90
Shapley-Shubik index	0.90	0.90
Banzhaf index	0.90	0.90

The explanation for the almost identical coefficients in Table 1 lies in the fact that the 1976 campaign-stop data fall generally between the predictions of the proportional rule and the other model predictions. If the 50 states and the District of Columbia are partitioned into large (20 or more electoral votes), medium (10 to 19 electoral votes), and small (9 or fewer electoral votes), for example, the proportional and 3/2's rules predict the percent allocations shown in the first two rows of Table 2. The actual percent allocations made by each slate in 1976 are shown in the second two rows and indicate that the candidates—especially the Democrats—made greater-than-proportional expenditures in the large states, but not as great as the 3/2's rule (or power indices) predicts.

Table 2. Percent Allocations

	Small States	Medium States	Large States
Proportional rule	30.9	29.9	39.2
3/2's rule	16.1	26.4	57.5
Democratic slate	16.1	30.4	53.6
Republican slate	27.1	28.9	44.0

There is thus a large-state bias, but not of the magnitude predicted by most of the models. By way of comparison, it is interesting to contrast the actual allocations of the Democratic candidates with planned "percent of effort" figures for the Carter-Mondale campaign in each state, based on a scheduling formula devised by campaign director Hamilton Jordan prior to the general election campaign (Schram, 1978, Appendix 3). Jordan's figures correlate highly with the proportional rule (0.96) but not so highly with the other model predictions (0.86 with each of the two power indices and the 3/2's rule). In fact, their correlation with the Democratic slate's actual allocations is not as high (0.84) as any model correlations in Table 1.

What happened? While Jordan's formula succeeded in giving each state more or less its due—a proportional allocation—this egalitarian ideal was abandoned in the actual campaign. I conclude that even the best-laid plans of candidates may be upset when a perverse institution like the electoral college dictates nonegalitarian allocations.

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The N-Prisoners' Dilemma: A Bureaucrat-Setter Solution*

TO THE EDITOR:

Orbell and Wilson (1978) have recently made a valuable contribution by analyzing the outcome of a symmetric N-prisoners' dilemma under majority rule, selfish dictatorship, and laissez-faire. Their central point is that which of the three institutions works best depends critically on the economic cost-benefit parameters of the game. As each of the three institutions was best for a certain range of parameters, no institution could be rejected as being inferior.

This note proposes a simple institution, the bureaucrat-setter, that is virtually always preferable to dictatorship.[1] Using the Orbell-Wilson definition of social benefit, the setter gives at least as great aggregate social benefit as dictatorship. Dictatorship can thus be rejected within the framework of the Orbell-Wilson model.

Our mechanism is as follows. One player-citizen is selected as the setter. The setter then proposes a policy. The policy specifies which individuals defect and which cooperate.[2] If the policy is approved by a majority of the voters, the policy dictates the outcome. Otherwise, laissez-faire prevails. (In fact the setter will always propose a policy that defeats laissez-faire.) It is crucial that the constitution forbid amendments to the setter's proposal.

When the social externality imposed by a player exceeds his private cost in eliminating the externality, our mechanism gives, for large N, the same outcome as the Orbell-Wilson dictatorship.[3] Specifically, the setter proposes that he defects and everyone else cooperates. All voters prefer this policy to laissez-faire, and the policy is enacted unanimously. The setter-

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*We thank John Orbell and L. A. Wilson II for helpful comments, and the Spencer Foundation and NSF for financial support.

[1] The "virtually" concerns a minor technical qualification found in n.3.

[2] A real-world situation close to the bureaucrat-setter model is found in school operating budget referenda. Here the school board—on the counsel of a bureaucrat, the school superintendent—sets a proposal that must be approved or rejected. (The school situation differs from the context of this note in that a prisoners' dilemma may not be present and in that the setter may have the option of calling additional elections if the budget is rejected.) Like Orbell and Wilson, this note is not concerned with how a society might come to select a particular institution (democracy vs. dictatorship vs. setter) or designate individual decision makers (dictator or setter).

[3] Except for the uninteresting case where an individual's personal cost from his own externality exceeds his private cost in eliminating the externality, the setter's preferred policy will be for him to defect and for others to cooperate. Orbell and Wilson define D as the defection benefit and C as the cooperation benefit. Let $Z = D - C$ and let S denote Orbell and Wilson's social fine (externality). All voters will prefer the setter's preferred policy to laissez-faire if $(Z + S/N) < S$. When $(Z + S/N) > S$ the setter will allow other players to defect. The range where dictatorship gives a superior aggregate benefit to that available from the setter is $S > Z > S(N-1)/N$. For any given Z, this interval vanishes for large N.

dictator gets close to the social optimum of all cooperate, whereas laissez-faire would lead to the worst possible outcome: all defect.

In the opposite case, when private costs exceed externality costs, the setter proposes a policy where he and one-half of the remaining voters defect and everyone else is forced to cooperate. This policy is enacted by a bare (and tyrannical) majority.[4] This outcome gets closer to maximizing social benefits than dictatorship, which continues to have only the dictator defecting, although the best outcome is laissez-faire.

Orbell and Wilson point out that when the externality is high relative to private costs, majority rule works best in that "all cooperate" is a Condorcet winner. But "one defect, $N-1$ cooperate" is very close to this optimum, especially for large N.[5] When externalities are moderate (less than twice the private cost), majority rule breaks down. There are cyclical majorities.[6] Cyclical majorities also prevail when private costs exceed the externality. Having a setter always avoids the cyclical majorities.

The avoidance of cycles is one justification for the use of "closed rules" in legislatures and "up or down" referenda such as school budget or bond elections. Our remarks above indicate that such setter processes are very much a form of disguised dictatorship that uses voting to maintain an illusion of democracy.

Many other institutional mechanisms might be investigated, including the newly recognized demand revealing processes.[7] Just as the setter has simplicity relative to majority rule by avoiding possible cycles, the setter mechanism has the simplicity of avoiding the heavy informational requirements of demand revealing processes.

In summary, we note that Orbell and Wilson have used the cyclical majorities that arise in their model as a rationale for the classical Hobbesian argument for dictatorship. More enlightened Hobbesians will want setters.

THOMAS ROMER
HOWARD ROSENTHAL

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[4] For example, Orbell and Wilson used a nine-person game. The setter there would allow himself and four other players to defect. They would have a net benefit of $D-(5/9)S$ that they prefer to the $D-S$ of laissez-faire. The losing minority would have a net benefit of $C-(5/9)S$, which is less than the $D-S$ of laissez-faire, since $D-C > S$ by assumption. The setter thus is not Pareto superior to the dictatorship; it is better only in the aggregate.

[5] In the single-peaked case, Romer and Rosenthal (1979) show that the setter can lead to outcomes far from the Condorcet point. This arises from the setter's preferences not being symmetric to those of other voters. Asymmetrics in preferences would also complicate the analysis of the Orbell-Wilson problem both for the setter mechanism and the institutions they examine.

[6] Indeed every policy is in the top cycle set.

[7] Demand revealing processes are discussed and referenced in the articles contained in Tideman (1977).

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Another Solution

TO THE EDITOR:

Avoiding cyclical majorities by introducing a "bureaucratic-setter" is nothing if not ingenious. In the first case discussed by Romer and Rosenthal (1978), though—externality exceeds private cost—the setter is a euphemism for a dictator. Romer and Rosenthal recognize this, but claim the setter's sole defection would be preferred to laissez-faire. Maybe so, but this fact does not sanctify the existence of a dictator who is uniquely exempted from the policies to which everyone else must adhere. Romer and Rosenthal never make clear how their setter is to be selected, but I venture to say that the selection process for most dictators is rigged.

How the setter is selected to form a bare majority of defectors in the second, more democratic case undoubtedly depends on the organizational context. In essence, I believe, a competition occurs for playing dictator in totalitarian countries, playing one of the defectors (e.g., being in the "elite" and thereby benefiting from tax loopholes, etc.) in democratic countries.

If the competition for being a dictator or defector is genuine, then one might argue that democracy sneaks in by the back door. However, this competition usually occurs only among a small group; moreover, it is subject to the same N-prisoners' dilemma, albeit at a much rarefied level. Thus, I believe, the bureaucratic-setter does not really obviate the problem Orbell and Wilson (1978) pointed out.

The problem of cycles is solved in practice by precisely the mechanisms of "disguised dictatorship" alluded to by Romer and Rosenthal. I would prefer the term "disguised arbitrariness," because the setter may more resemble a populist like Howard Jarvis, who was instrumental in the enactment of Proposition 13 in California recently, than a dictator in the more usual sense.

But note that Proposition 13 was probably not in a cycle, because it defeated the more moderate proposed property tax cut (Proposition 8) as well as no cut (the status quo). In fact, the referendum was not either-or, as most referenda are, but was effectively based on a limited form of approval voting (Brams and Fishburn, 1978). Although voters could not directly approve of the status quo, or the status quo and one of the propositions, indirectly they could approve of the former by not voting for either of the propositions, the latter by voting for just one; what they could not do, however, was register simply a preference for one of the propositions and not the status quo.

Since approval voting does a better job of guaranteeing the election of a Condorcet alternative (if one exists) than any other nonranked voting system—and elects the "most acceptable" of alternatives in a cycle when there is no Condorcet alternative—I would contend that it offers a *partial* solution to "disguised dictatorship/arbitrariness." That is, though approval voting does not always elect a Condorcet alternative, nor does it rid the world of cycles, it is biased in favor of the alternative acceptable to more people—and one they probably can better live with—than any other.

I would add that approval voting is a far more practicable solution than the complex demand-revealing processes cited by Romer and Rosenthal. Finally, I believe voters would feel more efficacious, and be less cynical about voting, if they could indicate which of several alternatives on a ballot were acceptable (including the status quo) rather than being forced into an either-or choice.

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The Uses of Expanded Majorities

TO THE EDITOR:

Romer and Rosenthal (1978) have shown that the bureaucrat setter is superior to dictatorship over the whole range of symmetric externality conditions. Although it produces the same outcome for all prisoners' dilemmas as dictatorship (marginally sub-optimal but equilibrium), it is superior to dictatorship in the non-prisoners' dilemma case. Thus, constitution makers operating under uncertainty about the world in which their institutions must operate can safely reject dictatorship in favor of the setter. The more democratic character of the setter (albeit a somewhat questionable democratic character) makes this an important finding.

In our original article (Orbell and Wilson, 1978), we showed that majority rule[1] can be relied on to produce optimal and equilibrium outcomes for some prisoners' dilemmas (opportunity costs of cooperation less than 50 percent of the externality), but that for all other conditions it produces a cycle. Might majority rule be confidently rejected in favor of some other institution in the same way that the bureaucrat setter allows the confident rejection of dictatorship?

In Figure 1 we have used the same conventions as before to define a prisoners' dilemma in which the opportunity costs of cooperation are more than 50 percent of the externality. As we have argued, majority rule (50 percent + 1 or, diagrammatically, $NR + 1$) produces a cycle for such a condition. However, should the decision

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*We wish to thank C. Edwin Baker, William Baugh, Robyn Dawes and—especially—Thomas Romer and Howard Rosenthal, all of whom gave us invaluable assistance with this note. We also wish to correct a typographic error in the original article that might have caused some understanding. Page 413, paragraph numbered 1, the sixth line should read: at the extreme *right* of the axis.

[1] We are talking here about the majority-determined choice among the whole range of possible outcomes. Notice that the bureaucrat setter involves majority-determined choice between laissez-faire and one other outcome—to be determined by the setter.

rule require the more inclusive $NS + 1$ before a decision is reached, it will be impossible to find enough voters to reject the optimal "all cooperate" outcome—which will dominate all others.

Generally, if an equilibrium solution is to exist, it must be impossible to find the majority specified by the decision rule to support outcomes to the left of "all cooperate." The proportion of the population required to prevent the success of such a "leftward" movement must be greater than the ratio of the opportunity costs of cooperation to the externality associated with defection.[2] Graphically, $\frac{NS}{NQ} = \frac{DC}{DN}$ and the required majority $= NS + 1$. If the majority required to adopt any particular platform is at least this large, an equilibrium outcome at "all cooperate" is assured. We can see, therefore, that $NS + 1$ in Figure 1 will produce the same "all cooperate" outcome as 50 percent + 1 where the opportunity costs are less than 50 percent of the externality, our "modest" case. And it will produce a cycle for the non-prisoners' dilemma case (where the opportunity costs are greater than the externality), as does 50 percent + 1. But, because it *eliminates* cycles in a range of prisoners' dilemmas whose opportunity costs are more than 50 percent of the externality, we can conclude—initially—that $NS + 1$ is a superior decision rule to 50 percent + 1 over the whole range of conditions. More inclusive majorities, it seems, are superior to less inclusive ones; constitution makers operating under uncertainty can safely reject 50 percent + 1 in favor of (say) 65 percent + 1.

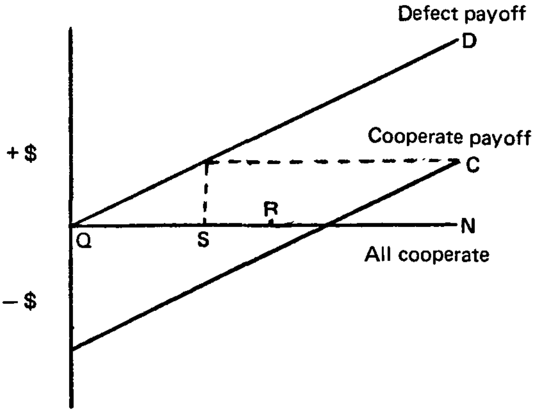

Figure 1. Payoffs as Function of Number Cooperating When Opportunity Costs of Cooperation Are Greater than 50 Percent of the Externality

There is a limit to how far we can usefully expand the decision rule, however. Let us consider what can be expected under a rule requiring *consensus* before a decision is made. For the range of prisoners' dilemma conditions it is clear that, under consensus, the optimal "all cooperate" will defeat the disastrous "all defect"—everyone will recognize that they will be, as individuals, better off under the former than under the latter. But there will be a range of outcomes (*NS* in Figure 1) for which some individuals will be better off as defectors than they would be under "all cooperate"; those individuals will be able, under consensus, to veto "all cooperate." In Figure 1, outcome *R* (for example) cannot be defeated by the outcome to its immediate right since the lone new cooperator will be able to veto the move, and neither can *R* be defeated by "all cooperate" since the 50 percent permitted to defect under *R* will veto "all cooperate." Conversely, "all cooperate" cannot be defeated by *R* since those obliged to cooperate under *R* will have the power to veto the move, as well. Similar logic applies to non-prisoners' dilemmas: while all will agree on "all defect" in preference to "all cooperate," they will *not* all agree on "all defect" in preference to any other outcome. Under consensus, a variety of equilibria is possible, only one of which is all cooperate.

But if the decision rule requires one individual short of the whole population to agree ($N - 1$), this problem will not arise. For all prisoners' dilemmas, all outcomes are dominated under $N - 1$ by outcomes to their immediate right (the single individual disadvantaged by such a move will be overruled) and the same applies to non-prisoners' dilemmas. Further, for all prisoners' dilemmas, the optimal "all cooperate" will be an equilibrium outcome since $N - 1$ does not permit collecting sufficient individuals to override it. *The rule* $N - 1$ *will produce an optimal outcome for all prisoners' dilemmas, something that cannot be expected from either 50 percent + 1 or any rule in between.*[3]

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[2] This rule is obviously limited to those conditions in which the opportunity costs of cooperation are less than the value of the externality. The case in which the required majority is 100 percent (consensus) is dealt with below.

[3] Romer and Rosenthal have pointed out to us that majorities larger than 50 percent + 1 applied to the bureaucrat setter will make no difference to the

The rule $N - 1$ will not, however, produce an equilibrium and optimal "all defect" for the non-prisoners' dilemma condition. While all outcomes are dominated by those to their right, "all cooperate" at the extreme right is dominated in its turn by "all defect" at the extreme left. $N - 1$ applied to a non-prisoners' dilemma does nothing to eliminate the cycle to be expected under 50 percent + 1.

Buchanan and Tullock (1962) have argued that expanded majorities involve expanded decision costs, and constitution makers will no doubt consider such costs in their institutional decisions. Although the logic of constitutional choice remains to be developed, increased decision or transaction costs will have to be counted against the losses to be expected from cycles under decision rules less inclusive than $N - 1$. Ignoring such considerations, however, it is clear that $N - 1$ is a superior rule to any less inclusive majority.

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Representativeness of Workers' Councils

TO THE EDITOR:

The article "Workers' Councils and Political Stratification: The Yugoslav Experience" by Sidney Verba and Goldie Shabad in the March 1978 *Review* is a welcome addition to the literature on Yugoslavia's system of "workers' self-management." It contains interesting new attitudinal data, as well as several stimulating observations about inequality in Yugoslavia. Unfortunately, the general point made, that socioeconomic and political factors are important determinants of participation and representation, is weakened unnecessarily by conceptual imprecision, methodological flaws, and contextual oversimplification. Perhaps a brief examination of these problems will elicit further information and clarification from the authors and will promote better planning and execution of subsequent works on the subject.

Two major problems are primarily conceptual. First, the stated aim of the article is to answer the question of "how representative of the working population are the councils" (Verba and Shabad, 1968, p. 80). Representation can mean many things, of course, but generally it implies a congruity of attitudes, characteristics, and/or actions of two sets of individuals: the representatives and the represented. Thus, one could answer the question by comparing (a) the social backgrounds of the members of the councils with those of their constituents (assuming that similarity of social experience is a crucial ingredient of representation); (b) the attitudes of the members and their constituents on salient issues (assuming that the congruity of attitudes is at the crux of representation), or (c) the explicit evaluations by workers in several enterprises of the representativeness of their councils (assuming that the workers' own definition of representation is the only important one).

Unfortunately, the authors take none of these approaches. Although they examine patterns of council membership as affected by two social-background variables (socioeconomic status and political affiliation), the crucial comparison of represented to representatives necessary for approach (a) is never undertaken. The social backgrounds of the councils as units are never juxtaposed against the backgrounds of their constituents, the enterprise work forces; instead, Verba and Shabad focus on individual council members (disaggregated by the two variables' categories, to be discussed below) and ignore those whom they directly represent—surely an inexact way to analyze representation. It might be noted that this problem is not solely one of the unit of analysis because the individual-level data could have been aggregated to the enterprise and council levels for the electorate and the elected, respectively. (Such an aggregation would have had the beneficial research bonus of permitting the analysis of the effects of enterprise-specific conditions on patterns of participation.)

The second conceptual problem relates to the dependent variable "participation." If one examines recruitment patterns, uses the term

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predicted outcome under prisoners' dilemma conditions, but that they will produce progressively superior outcomes in the non-prisoners' dilemma case. Under consensus the bureaucrat setter will produce the optimal "all defect" for non-prisoners' dilemmas.

"participation" to describe the individual result of those patterns, and even equates that result with "activism," then the implication is that formal membership on certain bodies (in this case, the workers' council) is empirically associated with taking certain actions in those bodies (e.g., presenting proposals, discussing the relative merits of different alternatives, etc.). However, equating formal membership with "participation in workers' councils" and "activism" is misleading.

Western studies of organization since Michels (1962) have shown that formal members of an organization fall into different groups according to level of activity and social-psychological involvement, the lowest levels of which are scarcely distinguishable from nonmembership. In addition, a growing Yugoslav literature on workers' self-management has demonstrated empirically not only that "decision making in the workplace tends to be technocratic in nature" (as the authors do point out on p. 82), but more importantly that the formal members of the council do not "participate" more or seem more "active" than certain key *non*-members, namely, the director and the director's immediate staff associates. These nonmembers not only dominate the enterprise's day-to-day decision making, but even within the confines of the meetings of the workers' council are the most activist and "participatory" of any group in the enterprise along such dimensions as proposal presentation and use of discussion time. The work of such Yugoslav scholars as Josip Obradović (1972), Branko Milosavljević (1970), and Miloš Vejnović (1974) clearly demonstrates this dominance.[1] If anything, membership on the council might be equated to *inactivity* in this context.

In any case, there are two conclusions to be drawn about the concept "participation" as used by Verba and Shabad. First, because the formal membership upon which their analysis is based does not "represent a more continuing and probably more significant level of activity" (p. 83) than nonmembership (especially for the managerial staff leadership), the use of the terms "participation" and "activism" seems imprecise, at best. Second, if it is to be used at all, the term "participation" requires a more sophisticated conceptualization in which there is allowance for variation of council members along a continuum and/or among different categories, perhaps following Barber's classic typology of legislators (Barber, 1965).

There are also methodological problems which are not so directly related to conceptual imprecision, but which are troublesome, nonetheless. A major problem is the reporting of the technical details of the study. Unfortunately, Verba and Shabad fail to present the sampling technique and the date of data-collection. The absence of these specifics casts unnecessary doubt on sample representativeness and the possible contamination of the data by political events. Similarly, the first independent variable, socioeconomic status, is presented as a trichotomy with unspecified limits operationalized from an unelaborated composite scale of education and income. The other independent variable, political-institutional affiliation, is also insufficiently explained. An important omission is the fact that formal affiliation to the Socialist Alliance (the middle category of the variable) is characteristic of nearly all workers in the enterprise, via their membership in trade unions, which are organizational components of the Alliance. This results in the variable becoming a near-constant for all but League members (who are operationalized out of the category). Thus, analysis using the "unaffiliated" category is somewhat deceptive in view of the category's miniscule numerical size in the population and probably in the sample.

In addition to problems with the variables, there is a weakness in the manipulation of the data. The juxtaposition of graphs may "highlight" the "effects" of the independent variables on the dependent variable (Verba and Shabad, 1978, p. 88n.), but the relative *degree* of impact of each independent variable is not clearly shown. It is not obvious from Table 4, for example, that socioeconomic status has a "more central role" in determining patterns of participation than does political affiliation (1978, p. 92), especially in view of the fact that the lowest score (52) among the three socioeconomic categories of League members if higher than the highest score (50) among these categories of the "general eligible population." Better than the use of graphs for the purpose would be regression or path analysis, in either of which the size of the relevant coefficients would unambiguously indicate relative statistical "impact" on the dependent variable.

The last major problem area is contextual oversimplification. Ignoring such mechanisms as referenda, recall, work unit councils, and "basic organizations of associated labor" (the last being compulsory in all firms since 1974), the authors omit alternate arenas of participation within the enterprise that also may provide

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[1] For an overview of these and other works, see Oleszczuk (1977).

"opportunity more effectively to influence decisions that affect everyday life" (Verba and Shabad, 1978, p. 94). This omission is to be particularly regrettable because data on the use of such mechanisms since the adoption of the 1974 Constitution are scarce. Earlier fragmentary data, e.g., Vejnović (1974) do suggest similar patterns of inequality, but the very proliferation of these forms in the past 15 years indicates a felt need for additional institutional structures in the enterprise. Also, this proliferation is part of the historical context of self-management to which the authors do little justice. One need not accept the official line of continuing progress to appreciate the constraints which historical developments place on individual behavior, as well as the predispositions engendered by those developments.

Finally, in focusing on general patterns of individual participation, the authors inadvertently neglect possibly significant variables in enterprise context. The importance of the social composition of the work force (sex, age, or nationality) is suggested by the comparative political participation literature and the studies of political culture in Yugoslavia, e.g., Bertsch and Zaninovich (1974). The dominant type of production technology has been found to affect at least perceptions of influence patterns, by Supek (1972). These and other enterprise-related variables could be examined if the data of Verba and Shabad were reaggregated as suggested above.

Although these conceptual, methodological, and contextual difficulties mar Verba's and Shabad's otherwise laudable study, it is to be hoped that further studies—more thoughtfully conceived, executed, and presented—will be forthcoming from them on the subject, for the workers' councils of Yugoslavia as laboratories for the study of political participation have received all too little attention from professional political scientists to date.

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TO THE EDITOR:

Thomas Oleszczuk's comments on our article are welcome. They provide an opportunity to clarify our argument and to explain the limitations of our data. They also allow us to reaffirm our conclusions which, we believe, are supported by our data.

Let us take Oleszczuk's points one by one:

1. Oleszczuk claims that we do not deal with the representativeness of the workers' councils. Our analyses deal with that explicitly and in rather straightforward terms. We demonstrate an over-representation of upper-status workers on the councils—using both a composite measure of status and a simpler measure of occupational level. We do this by comparing the social composition of our sample of workers' council members with our sample of members of the work force eligible for workers' council membership (i.e., those employed in the "socialist sector"). Thus we compare the representatives with the represented. We do not do this on a unit-by-unit basis, as Oleszczuk seems to want, because our data could not be aggregated in that way. Our sample was of individuals, not of enterprises or of workers' councils. It would be nice to aggregate by enterprise, but by no means necessary for our argument.

2. It is true that we use membership on a council as a measure of participation, ignoring the fact that members may differ in how they participate at council meetings. We are not unaware that there are many ways in which people participate, and mere membership on a council is but a crude measure. Nevertheless, we are justified in considering workers' council

members as more active than their nonmember counterparts. If we had other measures of their activity within the councils, we could have used it to see if the stratification patterns that distinguish members from nonmembers also distinguish active from inactive members.

3. Oleszczuk also points out that certain nonmembers of workers' councils—the director and the director's immediate staff—often dominate the council meetings and the enterprise. This is true. But the same studies that he cites also show that on certain matters—wages, personnel—councils do exercise influence. The council members are not as active and influential as the director and immediate staff, but they are more so than nonmembers. Our basic comparison is not obviated by his comments.

4. We should have given greater technical detail about our survey. The survey was conducted in March and April of 1971 in four Republics: Croatia, Macedonia, Serbia, and Slovenia. The survey was a multistage probability sample. Details are contained in Verba, Nie, and Kim (1978, p. 360). The same work contains more detail about our measure of socioeconomic status (p. 347).

5. Regression or path analysis might have been more appropriate for the substantive point we wanted to make though less appropriate for our data. Verba, Nie, and Kim (1978, p. 231) contains path analyses of the same data confirming our substantive points.

We are grateful to Thomas Oleszczuk for giving us an opportunity to clarify our paper.

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GOLDIE SHABAD

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Reference: Verba, Sidney, Norman H. Nie, and Jae-On Kim (1978). *Participation and Political Equality: A Seven Nation Comparison.* New York: Cambridge University Press.

EDITORIAL NOTE

The following articles have tentatively been scheduled to appear in the March, 1979, issue:

John C. Wahlke, University of Iowa, "Pre-Behavioralism in Political Science," 1978 Presidential Address

Philip E. Converse and Gregory B. Markus, University of Michigan, "Plus ça change. . .: The New CPS Election Study Panel"

Milton Lodge and Bernard Tursky, State University of New York at Stony Brook, "Comparisons between Category and Magnitude Scaling of Political Opinion Employing SRC/CPS Items"

Arthur H. Miller, Edie N. Goldenberg and Lutz Erbring, University of Michigan, "Type-Set Politics: Impact of Newspapers on Public Confidence"

Glenn R. Parker and Suzanne L. Parker, Miami University, "Factions in Committees: The U.S. House of Representatives"

Faye Crosby, Yale University, "Relative Deprivation Revisited: A Response to Miller, Bolce, and Halligan"

Stephen Taylor Holmes, Harvard University, "Aristippus in and out of Athens"

James H. Nichols, Jr., New School for Social Research, "On the Proper Use of Ancient Political Philosophy: A Comment on Stephen Taylor Holmes' 'Aristippus in and out of Athens' "

David L. Schaefer, College of the Holy Cross, "The Good, the Beautiful, and the Useful: Montaigne's Transvaluation of Values"

Eugene F. Miller, University of Georgia, "Metaphor and Political Knowledge"

Steve Chan, Texas A&M University, "The Intelligence of Stupidity: Understanding Failures in Strategic Warning"

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It is with sadness that I report the death of William C. Ware, an *APSR* editorial intern. Mr. Ware entered the graduate program at the University of Pittsburgh this fall, having compiled an outstanding record at Florida Atlantic University. He enthusiastically assumed the many duties associated with his internship. It was already apparent that he would make a significant contribution to the *Review*. He died October 24, 1978, from injuries suffered in an automobile accident. The loss of this promising young scholar has deeply affected us all.

BOOK REVIEWS

Political Theory and Methodology

Alienation, Praxis, and Technē in the Thought of Karl Marx. By Kostas Axelos. Translated by Ronald Bruzina. (Austin: University of Texas Press, 1976. Pp. xxxiii + 401. $12.50.)

Some tree made the supreme sacrifice in order that this book might be published. The sacrifice was in vain. While there may have been a need for the French original published in 1961, it is not clear why this English translation warranted yet another assault on some hapless forest. In a merely respectable undergraduate political theory course treating Marx, a student would be told that there is a very intimate, but very obscure, connection in Marx's thought among these three propositions: (1) Human beings, generically and essentially, are producers. (2) In the exercise of their productive powers, human beings alienate themselves from their own essence or nature. (3) This alienation is to be overcome, not by thought alone, but by practical activity that transforms the world in a very basic way.

That is approximately all Axelos tells his readers. Although the title raises the hope that Axelos will extend some of Marx's insights, or, failing that, just dispel some of the obscurity that afflicts Marx's thinking, that hope is not sustained by the book. Instead of analysis (as in the work of Avineri, Ollman, Tucker, Meszaros and McLellan, for example), Aexlos gives us largely a tediously repetitions paraphrase and summary of some important texts, mainly from the 1844 *Manuscripts* and *The German Ideology*. So, far from dispelling Marx's obscurity, Axelos merely reproduces it, calls attention to it, and in some cases even adds to it, as, for example, here: "The resulting being-in-becoming (which is also being as having-become), this natural-historical-human-being-becoming (which is also being as coming-to-be), is grasped by a unitary global, and holistic science" (p. 280). The paraphrase is interrupted periodically by undeveloped ruminations on Marx's place in the history of western philosophy. These ruminations sometimes converge toward the view that, "Marx continues the whole of Western metaphysics—principally its third period, European, modern thought, the philosophy of subjectivity as thinker and agent (a philosophy which is as well, by implication, a philosophy of objectivity)—and he does so by radically transcending philosophy precisely as search for the truth, as thought, theory, knowledge, and science" (p. 246). Throughout the book, but especially in the concluding chapter, Axelos does pose some important questions, among them whether we should share Marx's faith that alienation can be overcome, whether political authority would not still be necessary even in a communist society, whether Marx has really succeeded in closing the gap between theory and practice. But since these questions are now so familiar, an important book on Marx would not just ask, but try to answer them. Axelos is content, for the most part, to ask them.

I do not, however, want to suggest that Axelos, on the faculty of the University of Paris, has been altogether unilluminating. After reading his book, I understand clearly why French students pour out of the lecture halls periodically in search of paving stones.

Finally, the translator's introduction is excellent—indeed, an entirely adequate substitute for the book.

LEON GALIS

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Averroës' Three Short Commentaries on Aristotle's "Topics," "Rhetoric," and "Poetics." Edited and translated by Charles E. Butterworth. (Albany: State University of New York Press, 1977. Pp. xi + 206. $30.00.)

Readers of this *Review* might understandably wonder what significance such a work as this could have for the political scientist. Charles Butterworth anticipates this question at the outset: "Today, few people are interested in either Aristotle or commentary. . . . The passion for serious discussion about perennial problems has waned. . . . Even those still attracted to the philosophy of Aristotle are little inclined to study the commentaries by Averroës," although even Aquinas referred to him as *the* commentator. This neglect is unfortunate: "Like Aristotle, Averroës addressed himself to theoretical and practical questions of concern to human beings in all ages." Among all his writings, these studies of the logical arts best addressed "the problematic relation between philosophic thought, religious belief, and political conviction." The three treatises presented here for the first time in translation (they were recovered only 130 years ago) propose "ways of imitating or abridging correct reasoning in order to influence other human beings . . . especially with regard to political decisions and religious beliefs. . . . Intelligent awareness of such topics is important because of the constant influence they exert over thought and action.... Human beings are continuously seeking better ways to live with one another as fellow citizens, as members of different nations, or simply as associates.... Decisions . . . are tied to opinions about one's place in the universe and about the kind of life proper to man" (pp. vii–ix, 19–21).

In summary, Averroës' interpretation of Aristotle's method made a contribution to political philosophy, and Butterworth intends to continue the work of Leo Strauss and his associates (particularly Muhsin Mahdi in the field of Islamic thought) in recovering and restoring political philosophy to the discipline of political science.

In their medieval context, these brief treatises of Averroës (1126–1198) made use of Aristotle to suggest the superiority for public discourse of logic (extended to include rhetoric and poetics) over the more contentious Islamic tradition of dialectical theology. Thus Butterworth presents texts of historical as well as theoretical interest, verified, explained, and annotated by an impressive and devoted scholarly apparatus that runs to half the length of the book (the remainder containing his English and Arabic translations of the Judeo-Arabic manuscripts).

JOHN A. GUEGUEN

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Interpretations of Fascism. By Renzo De Felice. Translated by Brenda Huff Everett. (Cambridge: Harvard University Press, 1977. Pp. xiii + 248. $15.00.)

We can begin to understand the reasons for the controversy going on in Italy over the "interpretations" of Fascism generated by the works of Renzo De Felice by reading carefully the introductory and the concluding chapters of *Interpretations of Fascism.* (Michael A. Ledeen gives us an extended analysis of this controversy in his article, "Renzo De Felice and the Controversy over Italian Fascism," *Journal of Contemporary History,* 11 [October 1976]: 269–83.)

Of course, this is not to say that what lies between the first and the last chapters of this book does not merit our attention. This material consists of two surveys of relevant literature: Part 1 deals with the general interpretations of Fascism, both classic views and postwar theories; Part 2 concerns, broadly, "Italian interpretations of Italian Fascism." As could be expected, De Felice's coverage of the so-called "Classic Interpretations"–i.e., Fascism as Europe's moral disease; Fascism as a result of the historical development of certain countries; and Fascism as a product of capitalist society and as an antiproletarian reaction–is reasonably thorough and well balanced. The same is true of his treatment of selected "fringe" interpretations of Fascism–viz., the Catholic interpretation; Fascism as a manifestation of totalitarianism; Fascism as a metapolitical phenomenon; the psychological interpretation; the sociological approach; and the socioeconomic interpretation. These chapters will be useful to all students of Italian Fascism, and should be complemented by A. James Gregor's *Interpretations of Fascism* (Morristown, N.J.: General Learning Press, 1974).

The chapters devoted to the Italian interpretations of Fascism will be of less general interest–specialists in twentieth-century Italian intellectual and political history will find them valuable. The qualities which have made De Felice perhaps the foremost contemporary historian of the Italian Fascist era, together with his clear and concise style, give his readers an impressive panorama of Italian social and intellectual history that, while necessarily schematic, somehow seems "full" all the same. The only shortcoming I would note in this section is the lack of attention given to the more recent communist (e.g., Lucio Colletti), radical and extremist (e.g., the *Manifesto* group) interpretations of Fascism. On the other hand, De Felice does provide an accurate summary of Togliatti's important work, *Lectures on Fascism* (New York: International Publishers, 1976), originally given in Moscow in 1935 and first published in Italy in 1970.

Be that as it may, *Interpretations of Fascism* will win acclaim more for the historiographical, intellectual, and *political* attitudes toward the study of Fascism developed and defended in the introductory and the concluding chapters. Some of the points raised by De Felice in these chapters have appeared elsewhere in his works –see especially *Mussolini il rivoluzionario, 1883–1920* (Torino: Guilio Einaudi, 1965) on the "revolutionary" origins of the Fascist movement; and *Intervista sul Fascismo,* ed. by Michael A. Ledeen (Bari: Laterza, 1975) on the distinction between Fascism as a revolutionary mass movement and Fascism as an authoritarian regime.

In the present work De Felice tells us:

> Everywhere there is renewed interest in the problem of Fascism, its characteristics, social significance, causes, and historical interpretations. Especially among the younger generation, a growing number of historians has been joined in pursuit of the question by sociologists, social psychologists, political scientists, economists, and even philosophers (p. 6).

De Felice attributes this renewal of scholarly interest to changed political and intellectual conditions brought about by the "crisis" of the major ideologies which had previously governed attitudes toward Fascism. In a crucial passage—and one, of very few, that is notably ill-rendered by the translator, De Felice argues that because of these changed conditions

> discourse about Fascism also became less directly tied to the attitudes and to the passions of the years in which it evolved and, therefore, it became more difficult to misrepresent the desire to comprehend better the Fascist phenomenon as the desire to justify it, as happened quite often just several years ago. (*Le Interpretazioni del Fascismo,* 5th ed., Bari: Laterza, 1975, p. 15, my translation; *Interpretations of Fascism,* p. 7).

The dynamics of the new attitude toward the study of Fascism discerned by De Felice is the realization that there was nothing "demonological" about the occurrence of Fascism as a political phenomenon:

> The Fascist or authoritarian resolution in certain countries was in no way inevitable and conformed to no necessity. It was the result of many factors, all rational and all avoidable (p. 176).

De Felice shows us that Fascism came to power in Italy because of certain relative advantages over the available political alternatives. His *Interpretations of Fascism* demonstrates the fallacies of explanations of Fascism's success not based on this fact. And this demonstration compels us to re-think the questions and to formulate more realistic answers to them. It is a book, in the end, for all political scientists.

ROBERT C. NATALE

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From Mandeville to Marx: The Genesis and Triumph of Economic Ideology. By Louis Dumont. (Chicago: University of Chicago Press, 1977. Pp. ix + 236. $16.50.)

Louis Dumont is a French social anthropologist whose study of the Indian caste system, *Homo Hierarchicus,* has become a classic in its field. For the past ten years, Dumont has been interested in showing how a comparative anthropological perspective might shed light on the history of ideas and the ideological configuration of modern western society. *From Mandeville to Marx* is his most significant contribution to date in this ongoing study.

As a study in the history of ideas, *From Mandeville to Marx* is a troublesome book. Ostensibly Dumont claims to be investigating "The Genesis and Triumph of Economic Ideology." He wants to locate the place of economic thought in modern ideology and to show how "economic ideology" reflects the modern individual's perception of the social order. But for all intents and purposes, the study is little more than a rather disjointed attempt to tie together such figures as Quesnay, Locke, Mandeville, Smith, and Marx around a number of themes that seem to run almost helter-skelter throughout most of the book.

The book is divided into three main parts. In the first, Dumont tries to develop a "comparative approach to the study of modern ideology." In locating his work in relation to that of de Tocqueville, Maine, Tonnes, and Polanyi, Dumont tries to show how an anthropological perspective can assist one in understanding the "value revolution that separates traditional and modern society" through a study of the history of ideas. This is without doubt the most crucial part of the book and by far the most unsatisfactory. He introduces such key concepts as holism, individualism, and representations without further explanation and never adequately explores the complex relationship between ideology and actual social order. Moreover, he not only fails to delineate the relationship between economic analysis, moral philosophy, and political theory on an ideological level, but also he often appears to collapse an understanding of the way the world is with the way people have come to think about the world.

In the second part of the book, Dumont attempts to link together Quesnay, Locke, Mandeville, and Smith in terms of the idea of "the emancipation of the economic domain." In contrast to Part 1, Dumont's analysis of these writers takes on a very narrow focus that ignores important and relevant aspects of their work. For example, while analyzing Locke's

Two Treatises in detail, Dumont barely touches upon the ideological significance of Locke's philosophic or economic writings—a curious omission, to say the least, in a book concerned with "The Genesis and Triumph of Economic Ideology." Similarly, his discussion of Adam Smith is limited to Smith's theory of value, leaving other important dimensions of Smith's economic and political thought such as Book 3 of the *Wealth of Nations* or the entire *Theory of Moral Sentiments* outside the scope of investigation.

The third section of the book is probably the most perplexing in terms of style as well as content. It concerns Marx. Certainly Marx is an important figure in any study of the relationship between economic thought and modern ideology. But given the earlier theorists considered in the book, one might legitimately ask why Marx has been singled out as a representative of modern ideology and someone like John Stuart Mill has not. Undoubtedly, part of the answer lies in Dumont's interest in criticizing a Marxian perspective by analyzing the ideological foundations of modern totalitarianism. But still the question remains, why Marx? As in his presentation of the genesis and triumph of economic ideology in the work of Quesnay, Locke, Mandeville, and Smith, Dumont's explanation of its triumph in Marx's economics is narrowly conceived and confusingly developed.

By the end of the book, the reader is left with an uncomfortable feeling of uncertainty regarding what the book is about. Is Dumont seeking to provide an anthropological approach to the history of ideas or is he seeking to mark out the relationship between modern economic reasoning and the moral and political thought of a few individual theorists? Dumont's understanding of Marx is particularly problematic. While he claims to be pointing out some general conflicts that lie at the heart of Marxian analysis, the question arises whether the conflict lies in Marxian analysis or in Dumont's own comparative approach to the study of ideology.

The importance of *From Mandeville to Marx* lies in its very failure. In crossing narrowly constructed disciplinary boundaries, Dumont has gone where angels fear to tread. Like C. B. Macpherson and J. G. A. Pocock, Dumont has attempted to say something new and meaningful about the way we have come to perceive the economic foundations of modern society. But unlike Macpherson and Pocock, Dumont does not provide us with an approach to the study of ideology that enables us to look at the world in a new light. His work remains that of a social anthropologist somewhat out of place when analyzing the history of ideas. Thirty years ago another social anthropologist, Karl Polanyi, successfully crossed the barriers that separate the disciplines and gave us a classic interpretation of the rise of nineteenth-century English liberalism—*The Great Transformation.* Perhaps Dumont's work can help revive our interest in the approaches of other disciplines to the study of social theory and break down some of the parochialism that divides the academic community today.

EDWARD J. HARPHAM

University of Houston

The Psychology of Political Control. By Anne E. Freedman and P. E. Freedman (New York: St. Martin's Press, 1975. Pp. ix + 269. $12.95.)

The point of departure of this book is the study of political control (which, strangely is nowhere specifically defined). The book consists of a series of dialogues between a prince and his tutor (the authors) on the subject of how the future king can best control his subjects. The following coverage is extensive, but relentlessly one-sided. The authors discuss briefly "instinctual man" (Freud), and "humanistic man" (Maslow), but the rest of the book is devoted to the tutor's real preference, "environmental man" (Skinner). What follows are a series of analyses of such diverse topics as political socialization, political violence, and attitude change from the perspective of behaviorist theory, emphasizing schedules of reinforcement. What the authors do within this framework is done well. The discussions are clear, extensive, and interesting. The discussion of major resocialization is particularly well done and provocative.

Yet those who subscribe to the belief that introductory texts should present multiple points of view will find this book less than satisfactory. The presentation of one viewpoint may be supportable if both its liabilities and advantages are discussed in detail. Unfortunately, although the review of major research efforts in particular areas is closely and critically analyzed, the same high standards are not evident in discussions of the book's theoretical framework, behavioralism.

At one point the prince accuses his tutor of a bias toward behavioralism. The tutor admits to this and then goes on to note that, "were I of a different persuasion I would have little in which to instruct you, and you would have little to gain in attending to it." If this means that behavioralism is the only or even best path for analysis in political psychology the assumption does not progress past mere assertion.

The authors do consider, if only briefly, some possible objections to behavioralism, particularly the applicability of findings from highly controlled laboratory experiments to the everyday world we inhabit and analyze. The authors appear to accept Skinner's contention that there is no problem, but it is well to recall that even Pavlov's famous canine had to be forcibly restrained in order to become conditioned. The complex, ambiguous contexts that characterize most human life outside of the laboratory and total institutions, render the applicability of behavioral theory problematic. Because of this complexity, the operationalization of reward in any particular case is difficult. Given the extensive linkages involved in higher-order conditioning, coupled with the lack of total control, it may very well be that what operates as a reward for one person (group) may be a neutral stimulus to another, and a deprivation to still another. These possibilities can be of little comfort to those interested in prediction as well as post-hoc explanation.

Freudian theory has been criticized for its alleged ability to explain everything, but I had very much the same feelings about behavioralism after reading the author's lucid discussions of political behavior. Like other highly abstract, all-embracing logical systems, behaviorism may ultimately be more useful as an organizing framework than as a validated theory of any particular political phenomenon. Care should be taken to distinguish its role in both cases, an admittedly difficult, but nonetheless necessary task, especially in an introductory text.

Yet, even if the political control necessary to make behaviorist theory practical were possible, it would remain to be determined to what extent it is ethically defensible. Those with interests in democratic political theory are apt to find the treatment of this and related issues in this book woefully inadequate. In discussing political life, the authors' emphasis throughout is on control by elites. In this book citizens simply do not count for very much. One gets some idea of their importance by searching in vain for any discussions of political efficacy or elite accountability. Citizens are viewed throughout as objects to be manipulated, benevolently perhaps, but manipulated nonetheless. The authors' viewpoint appears to be that, since we are manipulated anyway, it might as well be done in the service of preferred values. Just whose values, and why these and not others should be taken as authoritative, is never given much serious consideration. The authors are not unmindful of the ethical considerations of political control and behavior manipulation, but their chapter on the ethics of control is by far the shortest in the book. It covers only six pages, half of which are devoted to discussions of the procedural drawbacks to the large-scale use of psychosurgery and drugs for political control (i.e., that they are inefficient).

Yet even given the possibility of total political control and its ethical defensibility, one would still wish to consider its effectiveness. To date the Twin Oaks community and a few total institutions provide a mixed record of accomplishment. One must also consider whether there is not an inherent contradiction, ultimately irreconciliable, between the goals and assumptions of democratic societies and the methods of behaviorism. Suppose our goal is the greatest degree of self-realization of individual potential for the greatest number in our society. Can we teach individuals to make choices by taking them away? Does this not present a classic double bind in which individuals are told they should be autonomous, while at the same time their basis for becoming so has been systematically removed?

This text is an excellent introduction to the behaviorist approach to political psychology. It demonstrates again that collaboration between psychology and political science is both possible and desirable, if not necessary. Whether the particular viewpoint presented represents the most fruitful approach to the study of political psychology remains to be seen.

STANLEY A. RENSHON

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A Psychological Examination of Political Leaders. Edited by Margaret G. Hermann with Thomas W. Milburn. (New York: Free Press, 1977. Pp. xii + 516. $17.95.)

If the souls of departed colleagues still peruse with irrepressible scholarly curiosity the pages of this journal, we can report to Charles E. Merriam that a hope expressed more than 50 years ago is beginning to be grandly realized. Wrote Merriam in *Four American Party Leaders* (New York: Macmillan, 1926), "In view of the fundamental importance of leadership in any community, and especially in modern democracy, it is of greatest consequence that studies of the qualities of political leadership be energetically and intelligently prosecuted. And I venture to express the hope that the necessary interest and enterprise will be forthcoming in the not distant fugure" (p. 100).

In *A Psychological Examination of Political Leaders,* Margaret G. Hermann with Thomas W. Milburn and 19 colleagues, predominantly psychologists, have focused more diverse concepts, methods, and findings upon the study of political leader personalities than may be found in any other volume in the history of political science. With their work the scientific study of political leadership takes a giant step forward in a way that provides fascinating alternatives for future advancement. At least this book should demolish the myths that political leader personalities are too inscrutable for scientific analysis or that scientific methods themselves are an obstacle to the development of the study of political leadership.

With appropriate recognition of the pioneering contributions of Harold D. Lasswell (11 page references), Alexander L. George and Juliette L. George (18 pages), James D. Barber (23 pages), and Rufus P. Browning and Herbert Jacob (7 pages), the authors clearly proceed into new conceptual and methodological territory. An example of a "new" variable that will greatly intrigue political scientists is the masterful analysis of "Birth Order and Political Leadership" presented by Louis B. Stewart (M1M) in the tradition of Alfred Adler (MM2FMFMM). Another example is the demonstration by Robert C. Ziller and colleagues that a combination of two variables, "self-esteem" and cognitive "complexity of the self," can predict electoral success and aspects of legislative behavior as well as performance on experimental tasks in a university laboratory.

Among other variables and variable clusters that are treated in the 17 chapters are needs for power, achievement, and affiliation (David G. Winter and Abigail J. Stewart); stress (Robert S. Frank); operational code (Loch K. Johnson); initiation of structure, consideration, and role retention (Ralph M. Stogdill and associates); 100 personality descriptors (T. W. Milburn); dogmatism (Gordon J. DiRenzo); attitudes and issue-orientations (Philip D. Stewart); concurrent organizational ties (William A. Welsh); optimism, cognitive complexity, and humanitarian ideology (M. G. Hermann); cognitive complexity, attitude toward force, general ideology, and levels of trust (Michael J. Driver); stress, uncertainty and anxiety (M. G. Hermann); and militarism, risk-taking, and nationalism (Wayman J. Crow and Robert C. Noel).

Among research methods employed are analysis of biographical statistics; questionnaires; interviews; self-observation; indirect observation by close associates; direct observation of immediate and filmed behavior; documentary content analysis; content analysis of nonverbal, paralinguistic, and psycholinguistic behavior; field studies of natural events; laboratory simulations of natural events; experiments; and others the results of which are subjected to multivariate statistical analyses. In addition, Daniel Druckman demonstrates the usefulness of a propositional inventory to integrate findings from a wide variety of studies on bargaining behavior.

Some of the most interesting results arise when different methods are combined and compared. An example is Milburn's discovery that personality descriptions by close associates came close to a leader's own self-description. Another example is found in the essay by Winter and A. J. Stewart wherein personal motive scores derived from content analysis of inaugural addresses are correlated with presidential performance ratings independently made by expert historians.

Margaret Hermann's contribution to this volume provides a model for coping with tasks of creative integration in an age of increasing scientific specialization. She arranges the otherwise disparate essays around three topics: methods of personality assessment, personality effects upon recruitment, and effects upon role performance. Then, appropriately and persistently, she introduces, explains, anticipates, reminds, compares, evaluates, and suggests. In a final chapter she provides unusual summary tables of the research techniques, types of political leaders, and types of personal characteristics that have been examined. As a result of her work and that of cooperative colleagues, the volume sparkles with multivariate, multimethodological, combinatorial potential.

Largely because these researchers have done their work so well, it is evident what steps should be taken next. The case for continuing the research initiatives introduced here is thoroughly convincing. The promise of extending leadership research to encompass more varied theories of personal characteristic variables might be linked to other major subjects of interest related to leadership such as representational role orientations (Heinz Eulau, John C. Wahlke, William Buchanan, and Leroy Ferguson) and ambition theory (Joseph A. Schlesinger). In addition, normatively applied scientists will sense the need to relate personality variables more explicitly to realizing greater measures of nonviolence, equality, and freedom. Basic scientists will hope that personality psychologists will take up the investigation of the more covert and possibly pathological aspects of the personalities of some political leaders, a task that has been begun by Sam Janus, Barbara Bess, and Carol Saltus in *A Sexual Profile of Men in Power* (Englewood Cliffs: Prentice-Hall, 1977).

Margaret Hermann and her colleagues in psychology, sociology, and political science have provided a model of transdisciplinary contribution to the scientific study of political leadership. If their example is followed by other colleagues in the social sciences, humanities, and professions, and if political scientists themselves concentrate more attention upon the study of political leadership, similarly remarkable further progress can be anticipated.

GLENN D. PAIGE

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Ideology and Superstructure in Historical Materialism. By Franz Jakubowski. Translated by Anne Booth. (New York: St. Martin's, 1976. Pp. 132. $12.95.)

This essay reads like one of the current crop of interpretations of Marx inspired by the recovery of Hegelian Marxist thought since the late 1960s. "Hegelian Marxism" means that current of socialist theory arising in opposition to the reformist politics and mechanical materialist or neo-Kantian philosophy of the Second International. Initially inspired by the political audacity of the Bolsheviks and by a Hegel revival already under way before the outbreak of World War I, it found its leading exponents in Georg Lukács, Karl Korsch, and (with hindsight) Antonio Gramsci. Although through the early practice and later writings of Gramsci it has had some influence on the formation of the Communist Party of Italy (P.C.I.), it increasingly distinguished itself from the Stalinized "Leninism" of the Third International. The next generation of Hegelian Marxists moved further away from reigning orthodoxy by incorporating a Freudian dimension into their critique of advanced capitalist society. And it was via the direct influence of Herbert Marcuse and his associates at the interwar Institute for Social Research that a student-based Left generated newer theorists concerned with unfolding the concepts of the founders of a more aptly-named critical Marxism and an audience for translations and new editions of older works by members of this school.

Ideology and Superstructure, self-admittedly derivative of the early Lukács and Korsch, especially the latter, focuses on a major theme of critical Marxism, that of ideology and class consciousness. It is a revised version of a dissertation written in the early 1930s by a youthful political activist, although it avoids the conceptual murkiness characteristic of many such works. While it may offer few new insights to those already acquainted with *History and Class Consciousness* and *Marxism and Philosophy,* or with the more recent writings of István Mészáros, Frederic Jameson, and Bertell Ollman, to name only a few, in a less expensive paperback edition it would provide advanced undergraduate and graduate students with an excellent introduction to the critical Marxist treatment of such concepts as base and superstructure, consciousness and being, alienation and division of labor, and ideology and false consciousness. Jakubowski shares the methodological emphasis of the critical tradition on the primacy in Marxist thought of the category of concrete totality. This enables him to grasp the complex unity of thought and action, appearance and essence, and form and content. He reproduces this tradition's concern with the essentially historical and social character of Marxian materialism, as reflected, for example, in the close tie between revolutionary theory and the uneven development of the modern workers' movement. This view of theory is related to the requirement that a *critical* Marxism unfold its concepts in light of its own development, including the history of the reception of Marx by the Marxists.

Jakubowski is somewhat more "orthodox" than many current critical theorists, especially in his refusal to abandon the base/superstructure opposition, which he shows to be no frozen polarity or externally imposed classificatory framework; and in his unwillingness to distinguish sharply between the views of Marx and Engels on the aspects treated. On the other hand, while he makes no effort to develop a Freudian dimension, there is a clear indication of its possible importance for contemporary Marxism. Insisting on the need for an "analytical social psychology," he observes that "ideologies do not emerge directly from the relations of production: they are produced by men. They are the products of particular desires, impulses, interests and needs, which for their part are biologically determined and then quantitatively and qualitatively structured by the socioeconomic situation. Ideologies are their rationalisation" (p. 55; the reference to "rationalisation" by no means exhausts Jakubowski's multifaceted analysis of ideology).

But Jakubowski, up to this point reviewed as the author of a lucid introduction to "Main Concepts of Critical Theory," was not a student of Herbert Marcuse's in the 1960s but of one

Fritz Belleville in the 1930s. Belleville is described in an "Introductory Note" (signed "C.G.") as a "national leader of the German Trotskyist movement and a member of the Frankfurt School (in particular he was a friend and disciple of Karl Korsch)" (p. 7). While the index of Martin Jay's recent history of the Frankfurt School contains no reference to Belleville, the introductory note situates Jakubowski in Danzig, in 1936, as a local Trotskyist leader. *Ideology and Superstructure,* published the same year, indicates that he, too, was a "disciple" of Karl Korsch. Toward the end of 1936, he was arrested and tried by a Nazi-controlled court. Eventually released, he settled in the United States, where he changed his name and, apparently, his politics. He died in 1970.

Jakubowski thus cuts a singular figure in the Left politics of his generation. A follower of Korsch, he sternly criticized Lenin but refused to abandon the vanguard party. A Leninist who praised *State and Revolution,* he wished to effect a synthesis of Lenin's and Rosa Luxemburg's views on the relation of party to class. A Trotskyist who now and then turns a point in the manner of "the Old Man," he studiously avoids any mention of him in this study. If some sympathetic readers will find Jakubowski's politics untenable (as he himself was apparently to conclude), they will also note his reiterated emphasis on "the basic humanist element in historical materialism," an element which he associates with a future renewal of critical Marxism in light of an expanded level of working-class development perhaps more characteristic of the present than of the period in which he wrote.

THEODORE MILLS NORTON

Vassar College

The Rage of Edmund Burke: Portrait of an Ambivalent Conservative. By Isaac Kramnick. (New York: Basic Books, 1977. Pp. xiii + 225. $12.95.)

Although Edmund Burke is generally recognized as the father of modern conservatism, a cloud of ambivalence has always cast a shadow upon any interpretation of Burke's political thought. Burke the defender of the American Revolution and critic of the French; the opponent of natural rights philosophy and the chief prosecutor of Warren Hastings for violations of the law of nature; defender of the aristocracy and author of a relentless attack upon one of the leading aristocrats of the age. Naturally, Burke's biographers and interpreters have dealt with these issues in various ways. The question is, have they uncovered the roots of Burke's ambivalence? Kramnick maintains that they have not.

"The central purpose of the book" he declares, "will be to clarify Burke's ideological posture" (p. xii). This clarification requires, paradoxically, an unswerving focus upon Burke's ideological ambivalence. Instead of "the Tory prophet," Burke appears as "the ambivalent radical." To put it another way, what has always been latently troublesome to other interpreters is, for Kramnick, an object of conscious scrutiny. These *are* "two Burkes" (p. 4). Because "his personality is the key to this reassessment," Kramnick intends this assertion to be taken quite literally. "It is the relationship between Burke's life, personality, and social thought that will be studied here. This approach will involve linking his biography and state of mind to his actions, writings, and speeches" (pp. xi–xii). If Burke's political and social thought veers off in differing directions, this is reflective of the inner tensions, guilt, and confusion surrounding his quest for personal identity (pp. 8–9). Through Kramnick, Freud, Erikson, and psychobiography have come to Burke.

To those who reject the legitimacy of this genetic approach to the understanding of political theory, let it be said at the outset that they will find little to their liking in Kramnick's book. Even those with greater tolerance may find the discussion of Burke's "anal aggression" and scatological references (pp. 184–87), or his latent homosexuality (p. 83 ff.) too much for their credence. It is rather disconcerting—if refreshingly honest—for the author to concede that "there is no solid evidence that can be produced here which would positively sustain the interpretation of Burke's sexual and psychic life offered in this book" (pp. 87, 122). Nevertheless, Kramnick's use of evidence and his general interpretive framework are, in my estimation, well within the parameters of a plausible and legitimate reading of Burke's political thought.

Basically, Kramnick argues that Burke's life and thought are permeated by a love/hate attitude towards the aristocracy and the bourgeoisie. Psychologically, this attitude is rooted in his "troubled relationship" with his father, standing as an authority fiture (p. 9). Following Freud, Kramnick identifies radicalism as "symbolized aggression toward the father." Thus, "the bourgeois Burke traced in this study is the rebellious son repudiating the traditional authority of the father; the aristocratic Burke is the dutiful son worshipping the father or longing for a father to worship" (p. 64). Sometimes Burke's critique of the aristocracy expressed this latent bourgeois radicalism; at other times, his attack on the Jacobins reflected, in effect, a self-criticism of attitudes and tendencies within his own personality which he sought to repress (pp. 155–61). "Burke's rage-filled indictment of bourgeois radicalism in his *Reflections,*" Kramnick suggests, "should not obscure the very real extent to which he was himself a spokesman for bourgeois interests in these years" (p. 157).

What the author calls the "bourgeois dimension of Burke" refers especially to the latter's "restless drive for upward mobility" and the embodiment of the values of "the self-made man" idealized by the emergent middle class (pp. 52, 63). Yet, Burke's intellectual association with the aristocracy represented at the same time an effort to escape from "the actuality of his middle-class origins and status" (p. 102). The conflict between the "ambitious Burke" and the "self-effacing Burke" thus "reveals the basic ideological ambivalence which is at the core of his politics and his being" (pp. 83, 93).

Kramnick presses the argument in two directions. First, he links this ideological-personality ambivalence to Burke's doubts about his "sexual identity" (p. 9). From this standpoint, the "bourgeois" elements of his personality are identified with masculinity, ambition, sexual aggressiveness, and avarice, while feminine passivity, intricacy, mystery, and permanence reflect the "aristocratic" aspects of Burke (p. 97). Kramnick employs this interpretive axis in providing a controversial but interesting reading of Burke's *Vindication of Natural Society* and his essay *On the Sublime and Beautiful,* as well as his concept of party and his attacks on the Jacobins and Warren Hastings.

Kramnick also attempts to integrate his view of Burke into a larger historical-theoretical framework, namrly, "the transformation," expressed as "the peculiar combination of love and hate that the bourgeoisie felt toward the aristocracy" (pp. xii, 193). Precisely why Burke is the "most important embodiment" and the most "dramatic personification" of the social conflict between the aristocracy and the bourgeoisie is not made clear. On the one hand, Kramnick cites Erik Erikson's general observation that in periods of social upheaval "men are swayed by alternating world moods" to support his conclusion that Burke's ambivalence "matched most perfectly the historical identity crisis then being experienced by the advanced societies of England and France" (p. 190). On the other hand, with the dogmatic assurance of a therapist diagnosing a patient, Kramnick often declares that, as a matter of psychological necessity, "wherever Burke turned he saw this social conflict at work" (p. 109); it was, in short, "a projection onto external reality" of a psychological perception from which he "could not escape" (p. 156). Both answers beg the issue, and neither goes further than psychological determinism and historical coincidence towards providing a *theoretical* understanding of how and why personality characteristics are bound up with class divisions and the dynamics of social change.

Somehow, the notion that "one of the great turning points in European civilization" manifests itself in the "conscious life strategy" of Edmund Burke (pp. xii, 139, 190, 193–94) comes perilously close to Hegelianism run rampant. We need to know less about how the world spirit flowed through Burke, and more about how his contemporaries expressed in *their* thought the historically-rooted ambivalence Kramnick locates in the social structure of industrial England. As a general observation, it appears to this reviewer that Hegel still supplies for too many Marxists the paradigm for the integration of social and individual consciousness. In any event, the historical, class-based axis of Kramnick's argument is, in this book, its weakest link; and yet, one feels, it is to the author the most important dimension of his overall interpretation. Nevertheless, this study casts a provocative spotlight upon the shadows of Burke's personality and thought, and Kramnick's interpretation merits both serious and critical attention.

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Liberalism. By D. J. Manning. (New York: St. Martin's Press, 1976. Pp. 174. $10.95, cloth; $4.95, paper.)

The ambitious aim of this book is to present an account of liberal theory that will comprehend all its manifestations. The author repudiates as "necessarily misleading" those characterizations of liberalism that focus on one or a few of the greatest liberal philosophers. "An accurate portrayal . . . includes all the major points made by authors claiming to be liberals and recognized to be such by their fellow liberals, plus the points of authors subsequently recognized as the intellectual ancestors" (p. 139). Naturally, then, one cannot expect a brief book to give an accurate portrayal. Some criterion must be invoked to decide which authors are to be featured. Yet the selection cannot be based on any insight into the essence of liberalism, for it is "not characterized by an unchanging essence" (p. 29; cf. pp. 32, 60, 139). Nevertheless, Manning has discerned a "common framework" comprising three "principles": equal protection of the laws and a balance of political power; concern for self-expression and self-motivation; the university of humanity together with dedication to universal progress. These principles are held together by a "symbolic form of vision" that is more "aesthetic" than "logical." Despite its "incoherence," this symbolic form is illuminating because it is "persistent," because it "characterizes the tradition" and even sets the "limits" (pp. 13, 31, 143). Or does it? These principles are "not advanced as the criterion by which genuine liberal principles can be distinguished," for "there is nothing which is indispensable to a tradition of ideological thought" (p. 29).

Having thus shown just how precisely the subject matter can be defined using his approach, Manning proceeds to illustrate the development of the liberal "way of talking politics" by sketching how a succession of writers advocated toleration (Ch. 2). He then turns to the question of the "unity of liberal ideology"—he is aware that the first two chapters have not yet demonstrated a unity. The unity turns out to consist not so much in the "symbolic form" as in the arguments liberals advance advocating resistance to tyranny and free government. This account of what unifies and distinguishes liberals is inadequate because Manning never contrasts liberal republican theory with the republican theory of the classical or medieval thinkers.

By seeking to understand liberalism in the light of a vague aesthetic "symmetry" shared by all its proponents, Manning does not do justice to the philosophers who stand at the core of the liberal tradition. The result is inevitably a kind of caricature: what comes to sight is a broad and varied but flat plain of discourse, lacking any standards by which one could distinguish the few fully worked-out systems of thought from the utterances of lesser "ideologists." Manning has some awareness of the danger. He knows that "it has been the objective of many liberal writers to demonstrate that their political conclusions logically follow from either incontestable metaphysical foundations or indisputable factual evidence" (p. 119). So in the fifth chapter he intends to demonstrate the absurdity of this claim by refuting, not Spinoza or Montesquieu or Kant's *Metaphysics of Morals,* but Spencer, Green, and Locke. The refutation of Locke—the only original thinker of the first rank he addresses—"is not difficult" (p. 138). It takes less than five pages, and the tone of the argument is revealed by the remark that "it cannot for a moment be entertained that Locke's political theory is perfectly consistent" (p. 124).

Manning can rest satisfied with his exposition because he begins with the dogmatic assumption that all liberal theorizing is "ideology" (p. 5). Ideology is "systematic" but "subjective" thought; it lacks "academic objectivity." More precisely, while ideology always claims to be based on an "objective philosophic, historical or scientific account," it is "mistaken" in this claim and hence always "incapable of logical coherence." Now why have the liberal theorists, who include great philosophers of logic like Kant and Mill, failed to manifest any inkling of this logical incoherence? The answer is simple: "their opinions are historically relative" (p. 138). The great liberal theorists of the past did not consciously sacrifice their intellectual integrity; they simply lived in a benighted age.

But what does this mean for us? How does Manning think that he and we can remain liberals and still maintain intellectual honesty? Is it because we know that we cannot live without some subjective ideology, and hence we frankly choose the easy way, sticking with the beliefs Daddy said were good? But human beings cannot leave it at following inherited prejudice: they need an answer "why." According to Manning, the purpose of all ideologies is to meet the need not through "explanation" but through "assertion" and "the control of man's political inclination" (pp. 133, 155, 157). Once it is accepted that this is *the* function of ideology, it became doubtful whether liberalism, when compared with more "assertive" ideologies, can or even deserves to retain our allegiance.

As Manning candidly observes, "this is not a liberal book" (p. 5). It does, however, reflect in a sobering way the current level of the self-understanding of liberal rationalism among some "objective academics."

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Policy Studies and the Social Sciences. Edited by Stuart S. Nagel. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. xiv + 315. $19.00, cloth; $5.95, paper.)

This volume has been put together as a symposium, one of a series sponsored by the Policy Studies Association. The contributions are extremely short, which at this still early stage of disciplinary cross-fertilization has definite advantages. The book offers a survey of possibilities, not extensive treatises to be taken whole and applied to some social problem. The contributions come from a broad collection of specialties; some represent the social sciences proper, law, philosophy, social work, education; some are pioneers in the new field of large scale evaluation research; and among them there is even a physicist concerned with the social consequences of technology. These brief chapters provide the reader with a quick kaleidoscope of kinds of possible policy contributions of scholars and researchers dealing with society.

A major concern is the utility of current social knowledge to policy makers. From reading the contributions in this book one would have to say that no substantive generalizations have been made about what people do, of sufficient strength or predictability as to serve adequately the needs of the policy maker. Yet throughout this volume there is considerable optimism that a variety of academic disciplines have suggested answers to the question of how we may direct our fate through the collective order. There is not, however, any evidence of that kind of basic knowledge derived from "pure research" analogous to research in the natural sciences.

Most often the proffered good takes the form of certain skills, ways of defining problems, or models for structuring the questions posed. For example, Levine, Musheno and Palumbo, after a rather faulty critique of what they call "rational choice models," formulate a policy impact model for analyzing criminal justice policy. Not too surprisingly, it employs public choice terms and perspectives, most easily identified with the policy theorists they analyze at the outset. The contribution is useful as a kind of satisficing model, given the structure and rules the actors work within.

There were other contributions I found of great interest and utility. Irving Janis elaborates on his most interesting studies of "groupthink" as a cause of "gross miscalculations by groups of policy makers." In so doing, he offers a superb example of a diagnostic approach to the analysis of policy making. He is careful, modest regarding claims, and constructive in that he suggests preventives.

O'Loughlin writes on geographic contributions and brings the "spatial dimension" to bear with new perspective on such familiar concepts as distributive goods (and ills), access to power, location options and equity. The political scientist should be pleased with the helpful convergences demonstrated here.

The offerings, to be sure, are mixed in quality. One wishes Eugene Meehan had curbed his ill-tempered and impatient dismissal of philosophy. His space could have been used very fruitfully to discuss the intersects of various current applications of the discipline to public policy, such as the analytic philosophers, the neo-Marxians, the theorists of justice, and others perhaps unknown to most of us.

Now on some important disagreements among the contributors. A very basic level of discussion concerns the epistemological difficulties in applying social science "knowledge" to policy problems. It is amazing how casually the social scientist tends to handle the questions surrounding the nature of the knowledge with which we work. Is it appropriate and useful for public policy? With all our techniques and tests, do we feel secure about the utility, significance and validity of such knowledge? These are critical questions and I must say it is a relief to find some thought and even worry about the matter, and attempts at definition and perspective.

Some contrasting positions could be characterized as follows: In the words of Daniel Lerner, "The social sciences have developed in response to new needs generated by rapid social change. Policy studies are identified as (1) applied research such as surveys, voting studies, (2) the decision-making process, (3) "futurism." Duncan MacRae, Jr., writes: "Sociology, more than political science, is receptive to applied research. It is familiar with applying valuative variables (social integration, social mobility, morale, social disorganization, prejudice, alienation, crime). *But it does not deal with the sources of political choice or the conditions of political feasibility,* and thus often falls short of fully effective application." (Emphasis added.) And quoting Warren Samuels, "Economic applications must recognize the structural and normative aspects of the system (legal and moral rules, economic institutions, bases of economic position and participation). Policy analysts must retain the policy or decision-making character of the subject matter; unfortunately, the social sciences have characteristically used deterministic models."

The selections emphasize the *decision-making purposes* to which social studies must be applied. And the typical ways of applying natural and physical sciences do not hold. The decision format, *if* you do this *then* thus-and-so will happen, cannot be stated with any great degree of certainty regarding human affairs. At best, the format might be a tentative: *if you do not wish this to continue, try doing this other.*

The social scientist cannot make soundly based predictions from present, let alone future, behavior. The achievement of particular effects through policy interventions is not currently a firmly predictable matter.

It seems to me that the Samuels citation suggests a major reason. When trying to derive knowledge from determinist (causal) models, the researcher tries to replace a model of *complex interactions* and a variety of many circumstances, with one of much greater sequential simplicity. Such a model obscures the potentially great variety of human interactions and frequent unanticipated factors. Thus in policy situations reactions to programs, public actions, or new laws and decisions always seem chancey. The clientele, or target groups will act in accordance with *their* perceptions of the situation, as to the particular context in which they find themselves, and this is often far removed from appreciation by the policy maker. This makes it impossible to use directly any small laboratory findings, particular research or overly generalized "patterns" of not-too-highly correlated findings.

Thus I would take strong exception to the following position expressed by Thomas R. Dye: "There appears to be a developing consensus that explanation must precede prescription. [We] must . . . search rigorously for the causes and consequences of public policy, to utilize scientific standards of inference in this search and to endeavor to develop and test propositions about public policy. . . ."

Certainly we should emulate the canons of science in our studies, but the perspectives and models we apply must be appropriate to the questions we need to ask. In this regard, the contributions of evaluation research offer greater promise as exemplified in the Freeman and Bernstein chapter. They indicate the purposes of evaluative research and emphasize its distinctions concerning targets, process, goal measurement, impacts, controls and comparison groups.

Without a doubt, Donald Campbell makes an important point when he says that social science contributes its research methodology rather than descriptive theory. Scholar advisors too often fall into the over-advocacy trap. Instead of extrapolating alone from past science and conjecture about decisions made, the analyst should use the implemented decisions to correct or expand existing levels of understanding. So far as prior analytic knowledge is concerned for social applications, says Campbell, our strongest findings concern interaction effects and not main effects. Therefore ours is *conditional* knowledge and "social experiment" applications should better increase our scientific return.

Finally, I would commend Serena Stier's contribution which urges thoughtful caution. Do not underestimate the persistence of the bias of commitments of the researchers themselves—even those who proclaim "value-neutrality." Perhaps she asks the impossible along with this: that social scientists acquire some humility in their claims at prophecy and principle in urging governments to intervene in so many aspects of people's lives.

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Jung and Politics: The Political and Social Ideas of C. G. Jung. By Volodymyr Walter Odajnyk. (New York: New York University Press, 1976. Pp. xv + 190. $8.95, cloth; $4.95, paper.)

Carl G. Jung remains a relatively neglected figure, and his "analytical psychology" is little understood not only among political scientists but also among people in North America generally. Sigmund Freud never had a more incisive or more profound critic than Jung; Jung appreciated Freud's special strengths as well as his key weaknesses, and proceeded to work out an alternative psychological system. Jung had an elusive and often poetic style, which has not helped ensure his impact on social science. As a writer Jung is often hard to follow, and up to now his conception of human nature, with its implications for social and political thought, has been almost wholly unexamined.

The miserable state of Jung scholarship has not made things easier. Huge volumes of Jung's work have been published almost entirely without editorial apparatus, whereas Freud's disciples have been sure to annotate elaborately even the most casual of his essays. The Jung family recently allowed the centennial of his birth to pass without the appearance of an authorized biography, and they have been reluctant to have discussed in print aspects of Jung's life which might conflict with bourgeois Swiss morality. (Barbara Hannah's *Jung: His Life and Work*, which was published in 1976, is the broadest biographical memoir to have appeared.) While the Freud family, at least as of the *Freud-Jung Letters* in 1974, has stopped its practice of tendentiously editing Freud's letters, the Jung family still insists on its power of editorial censorship.

A book on Jung's social and political ideas is unusually timely. Yet partly because of the rich opportunities of the subject matter, Odajnyk's

Jung and Politics is a disappointment. It is far too narrowly conceived, as it presents Jung's ideas in skeleton form apart from either the tradition of social philosophy or the history of ideas. Jung is a more interesting thinker than this book makes him appear, and he deserves fuller treatment. Jung's conception of the unconscious was very different from that of Freud, and they disagreed, for example, in their outlooks on normality and rationality; many of the ideas of post-Freudian writers prominent in social science were in fact anticipated by Jung. *Jung and Politics* offers us very few references outside of Jung's own writings, and under the circumstances the absence of a bibliography is hard to understand.

Unlike many other students of Jung's ideas, Odajnyk does not try to dodge the question of Jung's involvement with the Nazis. Freud had charged Jung with anti-Semitism before World War I, and during the 1930s Jung in Switzerland collaborated with Hitler's regime. Perhaps no other feature of Jung's life has so justifiably damaged his reputation among intellectuals. Odajnyk's chapter 6, "The German Case," presents some of the available evidence; he accurately describes Jung's decision to publish a paper in Nazi Germany, where he discussed the characteristically Jewish features of Freud's psychoanalysis, as "a stupid and callous act for which there is really no excuse" (p. 107). Jung was so naive that in later years he did not realize that his best defense would be to plead political stupidity.

Freud's own practical politics were not particularly admirable; his support of an authoritarian Austrian regime, and his attitude toward Mussolini, ought to leave his orthodox supporters uneasy. Although the longest chapter in *Jung and Politics* is a comparison of Jung and Freud, Odajnyk does not seem to realize how alike they were, especially in terms of their concrete political and social convictions. It makes little sense for Odajnyk to conclude his book with the claim that whereas the implications of Freud's psychology are "reactionary," Jung's conception of the psyche "could lead to a harmonious and democratic resolution of the political and moral conflicts of mankind" (p. 187). We need no more trots on Jung, but rather presentations of his ideas that make his work come alive; and then it should be possible, as in the case of Freud, for people of various ideological persuasions to find interesting points in his system of thought from which they can build.

For political scientists it will be disappointing to be told again, as in the foreword by Marie-Louise von Franz, that "to a great extent all political dissensions and conflicts are exteriorizations of inner conflicts. . ." (p. x). It would seem from this remark that Jung's loyal disciples have not gone much further than devout early Freudians. Harold Lasswell was a brave and eclectic pioneer in 1930, but by now one would hope for more sophisticated conceptions of the interrelations of psychology and politics. Odajnyk cites, without adequate criticism, the study of Hitler written by Walter Langer in 1943; while Roosevelt's government encouraged this scrutiny with Freudian concepts of a wartime enemy, it is not surprising that it refused to sanction a similar examination of Josef Stalin.

Although Odajnyk tells us he came to Jung after being disenchanted both with behavioralism and with Freud, he does not seem to recognize adequately that his calling as a political theorist obliges him to be more self-conscious about the uses of psychology for partisan or moral purposes. Instead of defending Jung for having created a "science" and not a "cult" (p. xiv), Odajnyk would do better to see how features of both played a part in Jung's psychology. Such a mixture would not be unique even in contemporary academic life.

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Human Nature in Politics. Edited by J. Roland Pennock and John W. Chapman. (New York: New York University Press, 1977. Pp. xi + 348. $17.50.)

Political theory can neither live with nor live without some idea of human nature. The difficulty, well illustrated by these essays, is that any thorough examination of the defining characteristics of human beings is so remote from politics that no avenue of return is left open except the vague generalization. To say with Aristotle, the hero of this volume, that sociability is part of human nature, is too general to provide a bridge to anything specific. If, however, human nature is treated only as it bears upon government, the discussion is likely to be hopelessly superficial, given our burden of anthropological, biological and psychological information. In the face of these complexities an extreme scepticism is justifiable. And one of the perennial responses to scepticism is an honorable retreat to history. That is why George A. Kelly in "Politics, Violence and Human Nature" suggests that if political theory is to retain any focus it must concentrate on the history of conduct and thinking that bears on government. The price of intellectual promiscuity is incoherence. It is very doubtful that Aristotle had all mankind in mind in his much-quoted passages on man's social nature in the *Politics*. Mankind is, however, the subject of analysis now and little of political interest can be said about all of us as we are all of the time.

These possible flaws do not mar Roger D. Masters' wonderful essay, "Human Nature, Nature and Political Thought." Here is a typology of the history of ideas not just about human nature, but also the ways in which the latter relate to notions about the natural world generally and the emergence of both from mythic origins. The ancients, cheerfully or grimly, saw *physis* as a stable given, of which society might or might not be a part. The moderns see human nature as peculiarly capable of transforming the natural environment, though they disagree about the malleability of society. The dynamism of biblical religiosity and the resignation of older religious traditions are shown to have a considerable bearing on all four of the archetypical political rationalists whom Masters discusses, Antiphon, Aristotle, Hobbes and Marx. With these four, politics is never forgotten and a formal structure of intellectual possibilities is skillfully erected. The advantages of selective history become clear when one turns to Marvin Zetterbaum's far less successful "Human Nature and History." He also tries to show the advantages of a stable, "normal" view of human nature as opposed to a "historicism" which denies it. Common sense rebels against the latter, and our feeling for "transhistorical identity" is unsatisfied when we yield to flux. Since the author concedes most of the case for change and mutability, science is simply asked to serve rather than offend common sense and its yearnings for spiritual security. Why should it? Will human conduct improve? No one here cares to talk about evil, which used to induce thinking about human nature. Nothing is said about evil's modern equivalent, the Rousseauistic individual, nature's stepchild, unfitted for the environment by freedom, unlike all other organisms driven to self-destruction and vice. Instead we get an upbeat Aristotle who tells us that since human beings are social by nature, episodes like German Nazism are mere aberration. What Lisa H. Newton and James C. Davies need is a good dose of Thucydides. The first argues that only some severe social deprivation could cause the Nazis to kill so many of their fellow citizens. To begin with, the Nazis thought of Jews as subhuman and nothing is more common than such perceptions among members of alien groups. What informs this sentimental nonsense is a psycho-philosophy of history. According to Davies, there are a set hierarchy of stages of human needs, from hunger to self-actualization, and means to their fulfillment. Society develops along the same lines from the extreme scarcity of punitive anarchism to the perfect abundance of civilized anarchy, a post liberal-democratic stage. Ontogeny and philogyny are identical and move in one direction. Society is single units for Davies, at least until the nineteenth century when massive groups such as classes also become actors. There are regressions and collapses occasionally such as Nazism, the product of physical hunger due to social dislocations. In fact, there is no evidence that hungry people joined the Nazis; in fact, quite the opposite is true. Condorcet would blush at such a theory of progress, and so should we. It is not radical, but suffers from all the defects that Donald W. Keim discovers in the countercultures and Lyman Tower Sargent discovers in radical approaches to human value. These are both sensible, well-written pieces. The former, however, does show how quickly the "relevant" goes stale.

Social Darwinism always used biology to urge us to try harder, and Pete A. Corning's "Human Nature *Redivivus*" is no exception. After an able survey of current evolutionary biology, ethology, and cybernetics, we are told that each one of us is genetically responsible for the survival of the species. The article was written before the publication of Dawkins' *The Selfish Gene*. In any case, our biological fate must also become our social task and we must avoid the destruction of the species by accepting intelligent leaders and by becoming more disciplined and cooperative. Sound as this platitude is, we do not need cybernetic theory to support it, nor is the lesson of genetics as simple as it appears here.

John W. Chapman's essay, suggesting that some anthropology might be useful, is welcome, but it only scratches the surface. The remaining essays do not deal with the main topic at all. The gaps in this volume are therefore considerable. Human beings as speakers of grammatical languages are never mentioned and neither is the impact of language on consciousness generally. This book is, at best, a modest beginning.

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Wittgenstein and Scientific Knowledge: A Sociological Perspective. By Derek L. Phillips. (Totowa, N.J.: Rowman and Littlefield, 1977. Pp. xiv + 248. $17.50.)

Although Wittgenstein's later philosophy has contributed both to the decline of positivism or logical empiricism in Anglo-American philosophy and to the emergence of nonpositivist analytic political theory, his work has had little visible impact on the still-dominant behavioral and implicitly positivist tradition of empirical political science. One major reason may be the hostility of some of Wittgenstein's interpreters, most notably perhaps Peter Winch, to developing any law-like generalizations independent of the cultural perspectives of the relevant political actors. In fact, however, Wittgenstein's work is at least consistent with the important empirical, quantitative work of a pragmatist (rather than positivist), generation of Key, Schattschneider and Pritchett. And if Wittgenstein's influence on empirical political science increases, it will provide a philosophical rationale for a return to this earlier version of a scientific, empirical political science.

For our profession, the chief merit of Phillips' thoughtful, first-rate study is his emphasis on just this connection between Wittgenstein's philosophy and social science. After first discussing Wittgenstein's life and thought and the recent conflict between positivist and antipositivist philosophies of science, Phillips asserts his most interesting argument: that Wittgenstein's later work provides the foundation for a middle position "between the *relativist* approach where [as, evidently in Thomas Kuhn and Winch] ... particular conceptual and theoretical systems ... are treated as locally sovereign, and the *absolutist* [including the positivist] approach where certain abstract ideal universal standards are imposed on all milieu alike" (p. 80, Phillips' emphasis).

Phillips recognizes that there are substantial cultural differences between different "language games," e.g., the discourse of particular subcultures or scientific traditions. But he persuasively insists that each of these is built upon a common, ordinary language which can be used to talk about such language games (p. 88). These ordinary languages themselves, of course, differ very widely across cultures; and some of these differences are based on particular arbitrary conventions and these differences must be empathetically studied before we can generalize scientifically. As Phillips insists, however, all *workable* languages, functioning cultures, and serious scientific traditions are in part built on nonarbitrary features of the situation—on Wittgenstein's forms of life—in which the language-using group finds itself. And crucially, Phillips claims that these forms of life are in turn connected to what Wittgenstein called "very general facts of nature." It follows both that we are not free simply to choose *all* the meaning of our words—independent of those we give other words—and that the nature of the world *does not causally* determine what our words mean. As I understand it, then, Phillips' position is that law-like generalizations in the social sciences are legitimate (just as Wittgenstein's repeated contrast between scientific and philosophic investigation suggests), but only if we distinguish between features of constraint and commonality which make various situations similar, and differences of culture, convention and meaning which make some generalizations difficult and treacherous.

Accordingly, Phillips asserts against Winch that rather than "the individual investigator or the subjects of inquiry" (e.g., the political actors), "it is *one or another* group of scientists" who will be "the final arbiter as regards the correctness of various scientific explanations" (p. 140, my emphasis). Here, however, Phillips does not go far enough. As I see it, Wittgenstein's point is not simply that a scientific community while "bounded by the facts of nature" is free to choose within those bounds (p. 135) so that among psychotherapists, "the 'right' explanation is the one that is accepted *by other analysts"* (p. 179, my emphasis). For in fact, Wittgenstein's position also provides for the *comparative* testing of our language games themselves (although not necessarily of singular propositions within any one game)—a position consistent with C. S. Pierce's convergence theory of truth. Here, I think, Phillips does not fully explore the point of certain observations Wittgenstein makes in *Remarks on the Foundations of Mathematics* which he himself quotes: For example, "The truth is that counting has *proved to pay" (RFM* I, 4, quoted on p. 25, my emphasis) and (with respect to correct calculating): "If you draw different conclusions you do indeed get into conflict, e.g., with society, and also *with other practical consequences" (RFM* I, 116, quoted p. 134, my emphasis).

In sum, Phillips does not sufficiently emphasize this more "realistic" side of Wittgenstein's philosophy, which offers a basis for a genuinely scientific and empirical, though surely not positivist, social science. On the whole, however, this is a minor reservation about a book that not only lays out a vitally important set of issues but persuasively points to many of the answers.

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L'âge de l'autogestion. By Pierre Rosanvallon. (Paris: Editions du Seuil, 1976. Pp. 188. 7,50 F.)

This is one of the few solid books–and very possibly the most instructive–written in the *autogestion* tradition (self-management, self-government) reborn in France in the "events of May" 1968. It deserves serious attention by American analysts of French politics, and even more by American political and social theorists, all of whom will find the work at once informative and provocative. In a word, here is a book which some perceptive publisher will find well worth an English translation.

Pierre Rosanvallon is a young university professor and union leader, editor-in-chief of *La CFDT aujourd'hui*, the monthly publication of the socialist-oriented French Democratic Labor Confederation. Perhaps it is the author's striking combination of labor union and university careers which gives his book a realism lacking in most other discussions of *autogestion* (French or otherwise). Whatever the reason, Rosanvallon's book has disturbed many of his ideological compatriots, first of all because of the assertion that the possibility of a successful French *autogestion* movement depends on a "global political guarantee ... in which the military factor is determinant. . . . The development of a political and military Europe is indispensable to the development of a *société autogérée. . .*" (p. 78). In the very last paragraph of the book he returns to this judgment: "The international constraint ... is constituted by the problems of defense; it is therefore this problem, with its roots and branches, which imposes the strongest technological constraints, in that only a society capable of defending its independence can hope to gain autonomy" (p. 180). Rarely has the hermetic French *discours* on the themes of self-management and radical decentralization begun with such a lucid recognition of the world outside and beyond Left-Bank rhetoric.

Above all, Rosanvallon is thus interested in establishing the realism of the *autogestion* idea. And while he fails ultimately to convince one of its relevance as a general organizing framework for society today, his is one of the most brilliant and incisive discussions of the possibility.

Rosanvallon distinguishes six different *autogestion* languages or modes of analysis (pp. 10–12). The first is a "technocratic" language, in which *autogestion* is essentially a model of decentralized administration, a sort of "democratized management." A second language is "libertarian" or anarchist, which rejects the state as such in order to exalt direct democracy against representative or delegated power. A "communist" language understands *autogestion* as the second stage of socialism, a "rational and transparent" society. The "council" language, in which *autogestion* is a system of rule based on a pyramidal structure of workers' councils, attempts to flesh out the image of a regime which is the "social government of associated producers." Fifth, Rosanvallon analyzes the "humanist" language, in which *autogestion* is not so much a structure of power as a "way of being." One version of this is the theory of the transformation of daily life. Finally, the sixth language is "scientific," a projection onto society of the laws of biological function, "the aspiration toward a society in which the functionality of social relations wins out over all forms of domination and hierarchy," a society which is formed more from the harmony of its constituent parts than from their contradictions.

Rosanvallon argues that all six *autogestion* languages today restate classic arguments and in this sense *autogestion* is "nothing but a new word" (p. 13). On the other hand, he argues, the old ideas are given a new content in "the practical elaboration of the concept of democracy as a function of contemporary technological, cultural and political exigencies" (p. 16). Rosanvallon says that, as with its beginnings in the 1960s, *autogestion* will continue to develop out of practice into theory rather than the reverse (an implicit criticism of the grand schemes of some of his contemporaries), and that this process will be the practical elaboration of five propositions (pp. 15–17): (1) *Autogestion* will be the rehabilitation of the political (that is, democratic) dimension of socialism, rejecting the positivism and economism of Soviet-type socialist societies. (2) *Autogestion* is thus essentially the problem of the contemporary conditions of democracy. (3) *Autogestion* is not limited to the problem of social appropriation of the means of production; it is rather the problem of social appropriation of power in society as a whole. (4) *Autogestion* is simultaneously a strategy and a goal, surpassing the revolution/reform problem in a perspective of continuous experimentation. (5) A *société autogérée* will be essentially political, and will understand itself as such, therein reducing the influence of economic power both in reality (i.e., in capitalist society) and as ideological camouflage (i.e., in Soviet-style societies).

Finally, Rosanvallon concludes that the key to a more thoroughgoing democratic constitution is a reconstitution of civil society as the core of political society as a whole, outside of and, where necessary, in opposition to state-bureaucratic power. Here he rejoins contemporary Anglo-American discussion of the future of liberal politics, demonstrating, in his text and in his references, an authentic understanding of the Anglo-American vision. Moreover, he possesses the rare capacity to integrate French and Anglo-American ways of talking about politics and democracy, and provides a useful explanation and critique of many of the fashionable French radical theorists (Gorz, Clastres, Foucault, Castoriadis, Laborit and others).

While Rosanvallon does not succeed in convincing the reader—nor does he attempt to—that *autogestion* is a coherent doctrine or theory which may legitimately claim to be a way out of the general crisis of political life in the Western world; at the least (and this is a lot), he demonstrates why *autogestion* and Marxism-Leninism are incompatible in theory and in practice. Therein he suggests why *autogestion* has proved such a powerful ideological weapon for the Socialist party in its battle with the Communist party for ideological dominance on the French Left. Here is thus another piece of evidence that the recent resurgence of French socialism expresses a popular desire for an authentic politics and a renewal of political liberalism as much as it translates a crude electoral calculation and a "totalitarian temptation."

RONALD TIERSKY

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The Great Rights of Mankind: A History of the American Bill of Rights. By Bernard Schwartz. (New York: Oxford University Press, 1977. Pp. x + 279. $11.95.)

Bernard Schwartz's book on the history of the Bill of Rights focuses especially on James Madison's contribution to its formulation and adoption. But he misreads Madison's role in taking the initiative in sponsoring bill of rights amendments, for he believes that that initiative represents a shift in attitude in Madison's thinking. Schwartz, moreover, applauds that evolution of Madison's view since it makes him the prime mover in the congressional chapter of the Bill of Rights (p. 162). I would say that Madison's initiative can be better understood in the following terms: first, to forestall any contemplated anti-federalist attempt to use a bill of rights as a smokescreen to revise the basic structure and powers of the new government; and second, to confine the bill of rights amendments to specific protections of traditional civil liberties rather than the kind of standard-setting function of bills of rights that the anti-federalists had in mind. Madison's determination to incorporate *his* bill of rights into the existing body of the Constitution rather than having a separate listing at the beginning or the end is a clear indication of his intention to weaken, not elevate its status.

Schwartz believes that Jefferson's celebrated exchange of views with Madison was important in the evolution of Madison's thinking from his original lukewarm position toward a bill of rights (pp. 115, 160). While Madison was surely lukewarm toward adding a bill of rights to the Constitution, there is nothing to suggest that he changed his basic position as a result of his correspondence with Jefferson. Consider the following: In his December 20, 1787 letter to Madison, Jefferson included restrictions on monopolies, standing armies and the suspension of habeas corpus among the kinds of protections he was recommending for a bill of rights. Madison consistently opposed such amendments as tending to undermine stable and effective government (although there had been such proposals from the state ratifying conventions).

Schwartz regards Madison's June 8, 1787 speech to the House of Representatives in which he sets forth his bill of rights proposals as "one of the greatest addresses in our history" (p. 164). He has no doubt that this represented a very salutary attitude toward the Bill of Rights on Madison's part. But he does not consider and does not even refer to Madison's remark which is perhaps most revealing regarding his intention: "I should be unwilling to see a door opened for a reconsideration of the whole structure [of] the Government—for a re-consideration of the principles and the substance of the powers given; because I doubt, if such a door were opened, we should be very likely to stop at that point which would be safe to the Government itself." Schwartz says that the Madison amendments were a "distillate" of the various proposals emanating from the state conventions (p. 165), but he never considers what the basis of Madison's distillation was. What Madison chose to separate out from *his* proposals were the whole set of major anti-federal proposals that would limit the power of the national government or otherwise alter the basic structure of the Constitution.

Schwartz points out that the Virginia Declaration of Rights has the defect, as far as bills of rights are concerned, of being written in terms of admonition, not legal command. "Not once is there a 'shall not' which–in legal terms, imposes an unmistakable mandatory restriction that the courts can enforce" (p. 91). Madison, according to Schwartz, took the crucial step of substituting the imperative "shall" and "shall not" in almost all of the proposed amendments drafted by him (p. 91). Schwartz is under the impression that the transformation of the moralistic "ought not" of the old bills of rights into the legalistic "shall nots" of the American Bill of Rights represented an elevating of the function of bills of rights rather than the drastic narrowing and lowering that Madison intended. Schwartz misunderstands the intention of Madison's substitutions when he says that they were based on the understanding that mere declarations and wishful normatives were not enough, and that the situation called for flat commands (p. 170). The Virginia Declaration of Rights stated that free government depends on a frequent recurrence to first principles, an explicit set of standards in terms of which a government can be judged and, if necessary, resisted. Madison would have denied that statements of first principles are an adequate substitute for well-constituted and effective government. He would have regarded them rather as threats to good government.

Schwartz asserts that the history of the Bill of Rights is "the history of liberty itself," and his book culminates (although it does not end) with Chief Justice Warren's remark, with which he seems to agree, that without the freedoms guaranteed by the Bill of Rights, our Constitution would be a sterile document, for we would have the *form* but not the *substance* of freedom (pp. 9, 193). Yet even a cursory reading of *Federalist* #84 would show that there is more to American liberty than the Bill of Rights, and that there is plausibility in Hamilton's argument (with which I believe Madison agreed) that the Constitution itself is a Bill of Rights. Schwartz claims that on the bill of rights issue the anti-federalists had the stronger case and that their writings were the more interesting and even the more influential (pp. 111–12). But if his book shows anything at all, it is that *Madison's* contribution is the more influential one. Madison's contribution, however, lay in circumscribing rather than elevating the meaning of the Bill of Rights for, as he believed, the security of American liberty rests in a constitutional scheme which emphasizes the vigor and capacity of government.

MORTON J. FRISCH

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Methodologies for Analyzing Public Policies. Edited by Frank P. Scioli, Jr. and Thomas J. Cook. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. 171. Price not listed.)

In recent years, political scientists have become increasingly active in the analysis of public policy, an academic setting earlier dominated by economists, operations researchers, and scholars in other disciplines concerned with particular areas of substantive policy. This development has reflected the desire of political scientists to become more relevant to policy makers. Correspondingly, it has related to the need of these policy makers for more politically sensitive policy analysis.

Political scientists have brought to policy analysis an understanding of the complexity of politics–of the multiple phases of any policy action, of the multitude of political influences at work, of the difficulty of politically sponsored social change, and of the evolutionary processes through which such change may be accomplished. This edited volume provides ample evidence of such political acumen. Scioli and Cook immediately remind the reader of the multiple facets of public policy and of the need to focus particular pieces of research in relation to conceptual thicket of substantive and methodological choices. Moroever, the various pieces of this work exemplify the complexity whereof they speak, focusing upon such aspects of policy as: socioeconomic influences, governmental spending patterns, agency personnel levels, citizen involvement, agency outputs, program evaluation, policy impact, social indicators, incremental policy change, and policy implementation. At the least, this variety of topics suggests the need to move beyond narrower economic frameworks in the analysis of policy alternatives. The broader perspective of the political scientist introduces political and social complexities which such narrower frameworks either ignore or incorporate only very imperfectly. Further, this perspective cuts across different substantive policy fields so as to pursue the development of general, "policy-relevant theory" in the service of more "effective social policy."

The promise lies here, but so do numerous difficulties. The narrower fields of cost-benefit and cost-effectiveness analysis, and other forms of policy analysis developed within operations research, lend themselves more easily to substantial methodological development. Both their focus upon a narrow range of variables and their simplifying hypotheses support this development, although even they are plagued with the difficulties of policy complexity and certain arbitrary characteristics of prescriptive analysis. Nevertheless, their problems are relatively simple compared with those associated with a broader "political science" orientation to policy analysis.

Scioli and Cook touch upon the core of this problem. On the one hand, methodology must be gleaned, in part, from the narrower economic/operations research perspective; hence, this work includes a piece on cost-benefit/cost-effectiveness analysis. On the other hand, diverse social science methodologies concerned with social indicators, program evaluation, legislative analysis, and experimental social innovation are also explored to suggest particular methodological concerns with the field of policy analysis. In many cases, they involve relatively recent methodological developments, such as path analysis or quasi-experimental design or they involve changing applications of existing methodology, as with time-series analysis or regression analysis. In many cases, these methodologies lack clearly defined realms of applicability, or where such are clearly applicable, they are hard to apply within the world of practice. Finally, there is no evident frame of reference which would provide analytic coherence in the coordinated use of such methodologies in the field of policy analysis.

This is not to deny the merit of developing methodologies for the validation of "policy-relevant theory." Nor is to to diminish the merit of these methodological readings. Instead, it is to suggest why they lack greater theoretical integration or comparability of sophistication and/or scope. In essence, they rather accurately reflect the intellectual fervent and disorder of a developing field of inquiry, in which an array of specific methodological techniques and concerns are being explored without an integrated frame of reference. It is noteworthy that the only piece which more broadly surveys an area of methodology and technique pertains to cost-benefit and cost-effectiveness analysis, which derive from the narrower economic frame of reference. As work proceeds, a broader integrating framework should emerge.

There are two somewhat disturbing aspects to the book. Many of the essays are entirely too sketchy and as a whole, they are written for audiences of distinctly different levels of expertise. Still, this is a useful work which presents an array of methodologies currently in use in this quickly evolving field of study. It joins other constructive works, sponsored by the Policy Studies Association, in spreading the word of these developments.

WAYNE PENN

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The Reason for Democracy. By Kalman H. Silvert. (New York: Viking Press, 1977. Pp. xi + 136. $8.95.)

Kalman Silvert's effort in this slender volume emerges at a time of crisis or transition in American politics, according to the author. The crisis which occasions this book is the realization that American democracy is in trouble because it no longer adheres to the values upon which it was founded and because "too few of us any longer understand [democracy] or its mandates, [and] because too few of us honor it in comprehending words or in supportive action" (p. xii). These cumulative failures contribute to the corruption of the nation's political institutions, rendering them unaccountable to the body politic. Consequently, the very consensus on which the polity rests is undermined. The solution to the problem of American democracy must begin with a reexamination and commitment to the true foundations of democracy.

Silvert argues that America was established upon liberal principles designed to guarantee to all citizens equal political status based upon the idea of community. Liberal thought in the eighteenth century was fundamentally social but has subsequently been corrupted by the intrusion of a utilitarianism which is inherently asocial.

Utilitarianism, by reducing politics to the adoption of policies calculated to maximize pleasure by minimizing pain, becomes technique. The art of politics, along with the possibility of community, is destroyed by such thought because each individual is busily absorbed in making calculations designed to avoid discomfort. Reason is subordinated to appetite in a reversal of the Aristotelian formulation of the relationship between theory and practice.

Without community the state becomes an instrument of the interests which control it. Discussion of a national or public interest is precluded by definition since it has no basis in reality because no such entity exists in fact. The result is that "consensus and legitimacy go by the board, law and lawmakers . . . are mocked, . . . the community loses its power . . . [and] the ability to force social instrumentalities to submit themselves to control [fails]. The country becomes anarchic" (p. 41).

Silvert is in search of methods for re-establishing community on democratic foundations. Therefore, he rejects solutions to problems of social, political and economic disarray proffered by those who reject democracy on the grounds that it is impractical or inefficient. Opponents of democracy can be found in the mutually antagonistic camps of those who see all problems as amenable to technological solution or those who view our present difficulties as deriving from the glorification of technology. Both groups err because they mistakenly suppose that technology has a power independent of human beings.

The errors of the proponents and opponents of technology pale in comparison to a third school of thought which Silvert labels the Doomsday Analysts as represented by Robert Heilbroner in economics and Robert Huntington in political science. Their respective analyses lead to the view that current political, social and economic problems are beyond our ability to control, implying that any effort at reform is bound to fail. In attacking these convergent schools of thought, Silvert has identified a serious problem in American cultural life: the loss of a sense of individual and social responsibility.

The establishment of a democratic community depends upon the acceptance of responsibility for thought and action by individuals, which in turn creates the possibility of holding political leaders accountable to the citizenry. The question confronting the country, according to Silvert, is whether "to reconstitute democracy or continue our submersion in elitist authoritarianism" (p. 105).

Limitations of space prevent a detailed treatment of Silvert's conception of the reconstituted democracy we so sorely need if we are to survive as a nation and fulfill the promise of the Founders. Freedom based on political equality becomes the centerpiece of the new democracy. Freedom, as used here, is the ability to become oneself. This can only occur in a system of democracy defined as "a process for creating completeness" (p. xiii). To Silvert, democracy has to be seen as a process because we can never be certain that our values and world view are correct or absolute. Democracy requires "a relativistic system of governance that has as its first principle the separation of what is changeful and secular from what is permanent and sacred" (p. 99). This formulation leaves much to be desired. Silvert's solution to the modern American crisis is silent as to how the separation of the permanent from the mutable is to be accomplished, much less how we are to know or identify which phenomena fit which category. We are urged to realize our individual uniqueness, our potential to become, within a system of beliefs and government where the only absolute is change itself.

The strength of this book lies in its optimism and faith in the power of reason to restore decency and civility to our national political life. Its value lies in its reminder that we have free will, that by the exercise of will we can insist upon the accountability of leaders and institutions. To do this, however, requires the somewhat Nietzchean task of committing ourselves to the absoluteness of uncertainty. Democracy, as Silvert suggests, is a fragile system of governance, and while an act of will is necessary, by itself, it is certainly not sufficient for the realization of democratic government unless ultimately, that is our only resource.

STEPHEN J. COHEN

San Jose State University

Bentham. By James Steintrager. (Ithaca: Cornell University Press, 1977. Pp. 133. $11.50.)

Despite its title, this is not a biography of Bentham (no full account of his life exists), nor does it give equal attention to the entire range of Bentham's published writings, even his political and juridical writings. The book focuses mainly on the much-debated question whether and, if so, when, Bentham became a committed democrat. James Steintrager has made full use of the Bentham papers and promises to bring fresh evidence to this study. Unfortunately the evidence he brings is not conclusive as to the strength of Bentham's commitment to democracy, and by itself it does not go a long way toward explaining the nature of this commitment. The manuscripts do show that Eli Halévy's chronicle of Bentham's political preferences in *The Growth of Philosophic Radicalism* was essentially correct: Bentham gave at least speculative consideration to democratic principles from the start, his first piece to recommend the radical platform was *Catechism of Parliamentary Reform* written in 1809, but only later in the *Constitutional Code* did he abandon "democratic ascendancy" in a mixed government for pure representative democracy.

Steintrager covers this ground well, though an excellent account (with the historical setting drawn in more detail) is available in two articles by J. H. Burns, general editor of the new *Collected Works:* "Bentham's Critique of Political Fallacies" and "Bentham and the French Revolution." Steintrager does add two things to the record, however. He demonstrates that Bentham's retrospective view of his naive youth is misleading; he was never as sanguine about the absence of self-interested resistance to reform as his recollections would have it appear. Steintrager also recasts the motives which caused Bentham to turn to democratic institutions. Halévy emphasized bitterness over the rejection of his Panopticon project, but Steintrager disproves this and argues instead that Bentham's motives were more complex: he was principally moved by increased fear of crown patronage and by the effect of the war effort on the English press (both purely parochial political considerations—a point Steintrager emphasizes throughout).

When the author turns to an interpretation of Bentham's ideas, the result is disappointing. The book sustains the view that Bentham's attitude toward democracy was simply not the focus of most of his writings and is not, therefore, the best focus for a study of his political thought. After all, it is not surprising that in an era of democratic constitutionalism Bentham should have taken these questions up; what is of interest is how broad a view he took of the institutions required to support good government. Radical reform was just one. Steintrager recognizes that Bentham never put full confidence in the institution of representation; the people always seemed to him at once "clamorous and unruly" and "obsequious." But Steintrager pays insufficient attention to what good government meant apart from popular control of rulers by means of elections. Suffice it to say that Bentham appealed more often to the idea of responsibility than to representation; popular sovereignty, he knew, makes every official responsible, not just elected ones. And responsibility is a matter of the organization of officialdom generally and of a proper view of public service. On these matters the book says very little.

The book suffers on another count. It is simply impossible to make a meaningful judgment about Bentham's thoughts on the aptness of the people to control or participate in government or on the place of "exceptional individuals" in government apart from a comparative perspective which this study lacks entirely. There are few references to other thinkers and these few are casual. Is Bentham's understanding of representation like Burke's, as Steintrager suggests? Burke's view and the precise similarities with Bentham's go unexplained (and it would require considerable argument to show how Bentham's men of moral aptitude are identical with Burke's great families). In any case, the point is not conclusively made because Steintrager ignores an alternative tradition of continental radicalism which sees this issue in terms of people versus intellectuals or experts. Briefly, there are diverse forms of elitism; it is not helpful to refer in a phrase to Nietzsche's masters in this context. The same absence of clear concepts drawn from intellectual history weakens the discussion (now common among Bentham scholars) of manipulation of the people by legislators. For there is a difference between changing behavior by means of laws and punishments and changing desires or character by means of myths, stage effects, and other methods that do not appeal to common understanding. Bentham's position remains unclear. Most importantly, the question of who controls whom and how has no importance apart from the political values Bentham hoped to serve.

On this last point Steintrager seems to take a stand: Bentham valued toleration and looked on the principle of utility as "profoundly liberating." This is a sympathetic and defensible interpretation, but Steintrager goes on to say that in the course of his intellectual development Bentham became increasingly dogmatic. This argument too is weakened, because Steintrager does not explain what dogmatism means here: does it mean that Bentham became more deductive and less empirical, or inflexible in his commitment to democratic institutions, or less aware of the limitations of the principle of utility, as the author suggests at different points? And, did he cease to value toleration?

In short, this careful study makes good use of documentary materials. But it does not provide a comprehensive view of Bentham's political thought and Halévy's remains the best discussion of the range and setting of Bentham's ideas.

NANCY L. ROSENBLUM

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The Quest for Control: A Critique of the Rational-Central-Rule Approach in Public Affairs. By Herman R. van Gunsteren. (London: John Wiley, 1976. Pp. vi + 162. $14.95.)

Describing, analyzing, justifying, and/or explaining why national policies—even national institutions—are not working as they were intended to do is both a popular and important subject. Van Gunsteren's book adds to the literature on this subject; it is a provocative, competent, if somewhat disjointed exposition of why national policies fail.

Van Gunsteren defines the problem largely in terms of the imposition of a certain kind of rationality on political life. This type of rationality is intended to lead to centralized control. Hence the title of thd book. Rationality, or what the author calls the "rational-central-rule approach," is (finally) defined in the next-to-last chapter: "Rational = phantasy, world construction; central = only I (or we), isolated from the rest of the body politic; rule = exclusive control" (p. 143).

Nów, those who are not very familiar with the political thought of Hannah Arendt, Sheldon Wolin, Michael Polanyi and Ludwig Wittgenstein may not find this definition entirely self-explanatory. Basically, van Gunsteren is critically examining the logical-positivist school of philosophy; in particular, he is criticizing the several recent and varied attempts to apply the kind of rationality inherent in positivism to the political problems of order and control. Thus a good part of the book is devoted to describing and then critiquing these attempts to impose a machine-like or cybernetic order on political processes, and to deal with political problems as though they were amenable to objective, "linear" solutions.

The notion of "planning," for example—so in vogue among bureaucrats and politicians these days—is severely criticized by van Gunsteren for precisely the above-named reasons: Decision makers believe that they can rationally plan the future! One is reminded here, as well as in his discussion of program budgeting as another example of simple-minded rationality, of the work of Aaron Wildavsky on these subjects. Indeed, one of the truly original aspects of van Gunsteren's book lies in its intellectual synthesis of such disparate political scientists as Wildavsky, Arendt, Wolin, Pitkin, and Lijphart. (It is no coincidence that the book was written at the University of California at Berkeley, during a year's leave of absence from the University of Leiden.)

This book should interest both political theorists and students of public policy. Clearly, the author is primarily interested in the nature of the political theory which allows for and informs experiments such as program budgeting and national planning. It is no surprise, van Gunsteren argues, that these experiments are doomed to failure, because they rest on a political philosophy that seeks excessive control and which thus has only limited application to political life. Its applicability is restricted by a number of limitations either ignored, avoided or dismissed by this approach: that political reality is complex, is characterized by multiple centers of power, and is deeply influenced by history and tradition. The rational-central-rule approach ignores pluralism.

Though it reads more like a series of essays rather than a well-integrated attack on logical positivism, this is nevertheless a thought-provoking book. Perhaps its major defect is that the author's suggestions for avoiding the political pitfalls he describes earlier sound abstract and utopian. For example:

> subjects should become responsible citizens. They too must shake off their belief in rational-central-rule, because it prevents them from actively participating in public affairs. It is also necessary that there should be a forum, a place of appearance, where citizens can reveal who they are and what they want, and where their responsibilities become apparent (p. 154).

It is admirable to see these suggestions slated once again, but one has the uneasy feeling that the necessary social, economic, and political conditions for fostering such a notion of democratic citizenship have virtually disappeared. In this sense, these Arendtian ideas unfortunately seem far removed from reality.

JEANNE NIENABER

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Just and Unjust Wars: A Moral Argument with Historical Illustrations. By Michael Walzer. (New York: Basic Books, 1977. Pp. xx + 335. $15.00.)

This is one of the remarkably few books of lasting significance to have emerged out of the Vietnam War and its aftermath. Stung by an awareness of how insular was the reasoning that underlay most of the moral argument about Vietnam, Walzer has undertaken a philosophic and historical inquiry into humanity's moral response to war. He begins from and repeatedly returns to those twentieth-century conflicts we are so familiar with—the two World Wars, the Israeli situation, Korea, Vietnam—but he insists that we challenge and enlarge our moral sensibilities by also immersing ourselves in meditations upon specific war situations of alien ages and cultures. As a result, unlike the major recent productions of academic political philosophy, Walzer's attempt to uncover the universal principles at the root of our moral sentiments is manifestly not a mere systematization of ideological predispositions with which he began and which were never subjected to serious doubt. Moreover, his argument attains a compelling seriousness because it arises from vivid accounts of actual political leaders, citizens, and soldiers, rather than from clever but naive examples spun out of professors' imaginations.

Walzer has come to be convinced that we can make sense of the human moral posture towards war only if we resuscitate the Just War Doctrine of the medieval schoolmen and Grotius. He hastens to add, however, that the doctrine must be placed on an altogether new footing. Originally, the Just War theorists elaborated on the rules governing international relations by starting from the idea that all people and nations participate in a world community indirectly ruled by God and directly governed by Natural Law, whose provisions guide the nature of human beings toward their fulfillment as a familial, political, and rational animals. This outlook laid emphasis on the *duties* individuals and states had to the social wholes through which they were fulfilled, rather than on the *rights* each had as an independent equal in relation to other independent equals. Walzer is certain that the historical deepening of morality requires us now to base all moral doctrine on the more modern notion of the primacy of individual rights. "The correct view" is that "states are neither organic wholes nor mystical unions . . . ; [that] individual rights (to life and liberty) underlie the most important judgments that we make about war" (pp. 53–54).

In substituting this new foundation, Walzer passes over in silence the notable fact that the political philosophers responsible for the theory of individual rights are unanimous in denying all but the most attenuated versions of the Just War Doctrine. A glance at their reasons helps to illuminate the scope and difficulties of Walzer's project. As he is well aware, the emphasis on individual rights is intimately linked to the notion that collective obligation derives from a "social contract" (p. 54). This means that prior to the individual's "consenting" to some kind of contractual union, one remains in a political and moral condition of radical independence. Given the scarcity of things, this is a condition of intense competition that leads almost inevitably to armed (and not illegitimate) hostility. Now, since states are endowed with no duties or rights beyond those of the people who compose them (p. 53), unless they enter into some kind of international contract, they remain in such a "state of nature." Even Kant, who tries to enrich his predecessors' interpretation and give a more cosmopolitan, less self-regarding account of the rights of man, contemptuously dismisses the Just War Doctrine. In Kant's case, the rejection arises not only from an insistence on the consensual basis of political obligation but also from his hopes for regulating war through international contractual organization (a "League of Nations"). Besides, Kant was characteristically fearful of arming governments with moral pretexts for unilaterally imposing their will on others. Since Walzer explicitly refuses to elaborate his own understanding of rights (pp. xv–xvi), one can only surmise how he responds to these difficulties. It appears, though, that he adopts some Kantian version of the rights doctrine and means to unravel its new implications in the vacuum left by our epoch's disillusioning experience with the futility of world government and the UN (cf. pp. 123, 220).

Walzer tries to minimize the danger of moral crusades by showing that the society of nations is even less perfect, less analogous to domestic society, than the older Just War theorists claimed; its rules therefore call for even greater prudential latitude in their enforcement. Aggression, as an abridgement of the basic right of nations to self-determination, is a crime that merits not only defensive but punitive war—in which any nation can join. Yet the punishment ought not to contradict the fundamental right. Even criminal nations should be left their independence, and only in the case of a regime which has given clear evidence of unlimited aggressive appetites can other nations intervene in the domestic political structure. It is also only in such "Nazi-like" cases that intervention on humanitarian grounds is legitimate (a nation's right to independence derives solely from its individual citizens' rights to life and liberty, but this latter liberty requires that peoples be allowed to take responsibility for the success or failure of their own political orders). There are certain other situations that justify the initiation of "unaggressive" war, and Walzer constructs out of his historical researches careful guidelines defining legitimate preemptive war, counterintervention, and aid to fighters of civil wars.

Part of what makes the discussion as a whole so convincing, however debatable may be some of its details, is the way Walzer illustrates how one can successfully deal with the particular circumstances that must shape the meaning of the moral principles he discovers. Indeed, the principles are not fully intelligible apart from concrete applications, and are therefore always subject to revision, without being altered at their core.

This spirit of prudence continues to be exemplified in the second portion of the book, where the rules governing the conduct of war are explored. Walzer candidly admits that nuclear war, with its diabolical requirement of a deterrence strategy that keeps entire civilian populations in a sort of permanent hostage, defies any conceivable principle of noncombatant immunity. But he maintains that this horrible consequence of our technology must not obliterate our continuing adherence to, and hard thinking about, the moral limits on conventional warfare. To discover those limits, Walzer deploys some striking wartime examples that demonstrate why utilitarian arguments (attempting to define the limits by an appeal to strict economy of violence) fail to explain what we perceive to be the strictness of noncombatant immunity. He then proceeds to show how reflection based on the sacred rights of individuals can make more reasonable and orderly the time-honored but hitherto often crude "rules of war," as well as how those rules can be recast as military techniques alter. In particular, he clarifies the moral significance of modern submarine warfare, blockade, terrorism, reprisal, and—perhaps most important of all—guerrilla fighting. It is unfortunate that in dealing with this last topic, on which we so urgently need fresh guidance, Walzer allows his passionate opposition to the Vietnam War to cloud his usual lucidity. The restrictions he tries to impose on the admittedly brutal counterinsurgency operations guerrilla war demands are so severe as to render them almost impossible; yet he himself has taught us repeatedly that we cannot regulate the conduct of armies by trying to lay down rules so unrealistic that they must be broken if war is to be fought.

Happily, however, the chapter on guerrilla war is an exception. In the end Walzer is even willing to investigate and define certain "supreme emergencies" in which "imminent and certain" threat to a nation's survival justifies violation of the moral rules; but he insists that these rare and awful occasions should be seen *as violations,* that they should produce a measure of guilt as well as sadness in those who must perform the violations, and that they should be so set off in our consciences that they are deprived of the status of regular precedents.

It is true that Walzer's treatise leaves nebulous its foundation—the nature of the rights to which it appeals—and that this leaves unanswered certain theoretical questions to which I have alluded. One would have to add that the problem is compounded by Walzer's raising, but failing to respond fully to, the challenges of "historical relativism" and amoral political "realism." In fact, he almost gives the game away when he admits that the fundamental rights he speaks of may be "invented" rather than "natural" (p. 54). Nevertheless, leaving this confusion aside, Walzer's obscurity is intentional and admitted (p. xv): it is not his business to confront Machiavelli. In this academic journal a reviewer is permitted to wonder whether the full doctrine of "Just Wars," and even the sanctity of the self-determination of nations, must not be founded on a richer conception of man's duties than is derivable from the theory of individual rights (cf. p. 134 n. and p. 254). But Walzer's book aims at influencing political leaders and citizens in an age where human rights are the respectable source of the sacred, and it is to his credit that he shows awareness of the appropriate horizon within which such a book ought to speak. I conclude that this treatise is a worthy heir to the tradition of Vitoria.

THOMAS L. PANGLE

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The Polarity of Mexican Thought: Instrumentalism and Finalism. By Michael A. Weinstein. (University Park: Pennsylvania State University Press, 1977. Pp. ix + 126. $11.95.)

The search for a new social morality, a new definition of human purpose in a rapidly urbanizing and industrial milieu, has been an important aspect of Mexican intellectual life since the Revolution of 1910. In their quest for a just society, Mexico's most important twentieth-century intellectuals adopted the idealistic western postulates of equality and democracy. Their libertarian *weltanschauung* challenged the materialistically oriented, authoritarian and self-serving hierarchical organizations that emerged from the nineteenth century and the Revolution as a result of a national search for material prosperity and a resurgent positivism. In recent years, growing national economic problems, the failure of the West to live up to its ethical ideals when the latter conflicted with its economic and geopolitical advantages in the Third World, and the violent suppression of internal dissent by the government, have caused the hopes, the optimism of an older generation, to give way before a more critical, neo-Marxian view of the nation's dilemma and the world order.

The author of *Polarity* might not agree that a significant neo-Marxist shift has taken place among younger Mexican scholars. This essay examines what he calls "finalism," a libertarian philosophy developed by Antonio Caso, Jose Vasconcelos, Samuel Ramos, Leopoldo Zea, Octavio Paz and others which challenges authoritarianism and hierarchy, "the ideologies and institutions of the contemporary industrial powers." Finalism is posited as an original body of thought which rejects liberalism, Marxism and traditionalism while advocating "rebellion, equality, a hyper-individualism and charity." Its concept of freedom contests the "domination, alienation, atomization and powerlessness" brought about "by competition between organizations and hierarchical nations fighting for wealth, power, influence and self-righteous legitimacy."

Ramos made a major contribution to Mexican finalism when he rejected the prevailing mode of echoing European ideologies and values. Ramos, in perceptions remarkably similar to those later postulated by Frantz Fanon, identified European imperialism as a causative factor in a far-reaching inferiority complex among the peoples of the less-developed countries. For Ramos the *pelado* or lumpen proletarian was the ultimate manifestation of Mexico's inferiority syndrome.

A period of consciousness-raising among the Mexican intelligentsia then followed. The cosmic race and utopia of Vasconcelos and the *pachuco* in *The Labyrinth of Solitude* by Paz were logical products of this search for a new identity and self-understanding. The social sciences became integral parts of Mexican philosophy. Analysis of Mexico's internal authority structure and social hierarchy originating in the nineteenth century occupied Zea in his early years. In his more recent work Zea joined Caso in examining the external complexities confronting Mexico. Caso was deeply concerned with the rise of western totalitarianism in his later years and hoped that democratic institutions could evolve through international cooperation and the development of respect between peoples.

Zea shares Caso's trepidations regarding the external forces that impinge on Mexico and joins him in the hope for a just and democratic future. He reasons that many marginal societies, including Mexico, accepted the western postulates of democracy, but the western powers themselves violated that ideal when confronted by an upsurge of progressive and nationalistic political movements that threatened imperialist business interests and existing political-military alliances. Now the West is exhausted and demoralization has set in. Zea hopes this will serve as a leveling force and that in the future all cultures will work together as equals to solve world problems.

The ideas examined in this study anticipated the thought of Gandhi, Fanon and Nehru. The intensive introductory synthesis is somewhat unclear until the contributions of individual thinkers are explained in the chapters that follow. The author is at his best in the presentation of Zea and satisfactorily treats the complexities and evolution of Caso. The utility of the work is narrowed by the isolation of Mexican developments from parallels in other parts of Latin America and Asia. Timely comparisons would have enhanced its contribution considering the importance of the themes under analysis. More attention to developments at the National University and El Colegio de Mexico in the last ten years might have served as cautionary notice for the assertion that Mexico's leading intellectuals have "eschewed" liberalism or Marxism. The duality of positivism and finalism, while capable of causing great imbalance, is used to excellent advantage.

This is a useful work for scholars interested in the thought of Mexico's greatest philosophers. It is a significant step toward giving the Mexicans the recognition among world scholars that they deserve.

JOHN M. HART

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Autonomous Technology. Technics-out-of-Control as a Theme in Political Thought. By Langdon Winner. (Cambridge, Mass.: MIT Press, 1977. Pp. x + 386. $17.95.)

"Technology," Langdon Winner asserts, "has been a central theme in political thought for the past two hundred years"; yet despite this, he argues, "technology itself has seldom been a primary subject matter for political or social inquiry" (p. 2). In *Autonomous Technology* Winner seeks to put an end to this anomaly by discussing technology as a central—indeed, *the* central—problem of contemporary political theory.

This book is not, however, a systematic, definitive survey of how technology has been treated—implicitly or explicitly—in the total compass of recent political theory. Winner's concern is to deal descriptively and analytically with one approach to the interface between technology and political theory, the idea of "autonomous technology," "the belief that somehow technology has gotten out of control and follows its own course" (p. 13). This view received classic expression in Jacques Ellul's influential book, *The Technological Society* (New York: Knopf, 1964). Though Winner explicitly differs with Ellul on many points, he essentially agrees with Ellul in his basic approach to the problem of technology and human freedom. *Autonomous Technology* constitutes the most sophisticated attempt yet made to defend Ellul's highly controversial position and to place the argument within the context of contemporary (especially Marxist) political thought.

Winner's analysis and argument is complex and scholarly, and is elegantly and calmly articulated. *Autonomous Technology* is written in English rather than graduate-schoolese. But this characteristic—one of the book's great virtues—is also the source of its major defect. Though Winner is always careful to state his propositions clearly and to quality them adequately, the quality of the rhetoric often hides gaps or simple shortfalls in logic.

No brief review can do justice to the subtlety of Winner's discussion. Winner seeks to explicate the views of others as well as to state his own, moving easily (sometimes too easily) from one mode to the other. But, if one can summarize it without oversimplification, Winner's argument seems to be: (1) that technology has become self-augmenting and its consequences cannot be controlled; (2) that these consequences radically affect human life and political institutions; (3) that the problem is not technocracy in the sense of domination by technicians, but technocracy in the sense of domination by technology itself; and (4) that although it may be possible to choose among tools (the standard argument of those who reject "technological determinism"), we have lost the ability to reject technology in toto.

Put in a different way, his argument is that once we choose to go the technological route to human progress, we lose control of our lives, and democracy as self-rule becomes impossible and meaningless. "Technology is now a kind of conduit such that, no matter which aims or purposes one decides to put in, a particular kind of product inevitably comes out" (p. 278). Even such attempts to "control" technology as technology assessment, "alternative technology," etc., are futile since they also involve technology and such technological criteria as "efficiency."

Here is both the crux and, in my view, the fault in Winner's position. Following Ellul, Winner not only aggregates and reifies the concept of technology but makes technology equivalent to all purposive rational action. In so doing he creates the foundation for an essentially unfalsifiable set of propositions. All societies use technology in this largest sense in order to affect their social and physical environments and to condition their futures. (It is difficult even to imagine the possibility of a society which does not, one which is based on primarily nonrational, subjective, random, unstructured activity.) To say that we have lost our ability to deal with the problems of choice and agency (which—he correctly argues—are the essence of both normative and descriptive political thought) because, although politically we can have some control over particular technologies and their uses, we cannot escape technology as a whole, is the equivalent of saying that we are unfree because we cannot control—or reject—society as a whole but can only make marginal inputs into the flow of history. It is to complain that we are not God.

Happily, unlike most political scientists, Winner is not a moral auto-castrato and ends by suggesting that we ought to contemplate "dismantling . . . systems" and engaging in at least "epistemological Luddism" (p. 330). But he overtly refuses to specify what operational political consequences this implies, and his rejection of even the tentative means to control technology suggested by its critics would seem to amount to a counsel of despair.

Even if one does not accept Winner's position, this is a book which deserves serious reading—and rereading—by all those concerned with the shape of modern politics, whether or not they have ever consciously thought of technology as properly a major concern of political philosophy.

VICTOR FERKISS

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American Politics

The Implementation Game: What Happens After a Bill Becomes a Law. By Eugene Bardach. (Cambridge, Mass.: MIT Press, 1977. Pp. xi + 323. $17.95.)

Following the lead of Pressman and Wildavsky (*Implementation*), Bardach addresses the issue of how the varied and numerous program elements needed to put legislation or other social programs into force are assembled, directed, and controlled. Because the elements themselves are in the hands of different interests and groups having their own needs and agendas, the study of implementation is in part a study of politics with some of the same methodological and theoretical difficulties. Far too little study has been given to the processes of implementation as compared with the legislative process, and this book contributes to restoring some balance.

Eugene Bardach begins by examining implementation from the perspectives of pressure politics, massing of "assent," administrative control, and intergovernmental bargaining. He takes note of the complexity of joint action, but settles for a model in which he explores implementation through the metaphor of the "game." He argues that because control is exercised through bargaining, persuasion, and maneuvering under uncertainty, it resolves into strategies and tactics. Thus, implementation is described as a "playing out of a number of loosely interrelated games whereby these elements are withheld from or delivered to the program assembly process on particular terms."

This is both an important and a frustrating book. It is important because it focuses on the complexity of translating seemingly simple social policies into action and the importance of examining the motivations, assumptions, incentives, and contextual variables that affect the assembly process of any program. Although the implications of this type of analysis are rather pessimistic, it makes clear that understanding social policy requires knowledge of the behavioral systems of groups and individuals. The book is frustrating because of its descriptive orientation and the absence of greater effort toward a theoretical statement. Instead, the author maintains that the relationships are so numerous and diverse and the world so fragmented and disjunctive as to make any general theory unattainable and unrealistic.

When Bardach discusses the implementation of mental health reform in California (the subject of his earlier book on the *Skill Factor in Politics*)—and particularly how Frank Lanterman played not only the role of legislator but administrator in seeing to it that his pet bill was actually implemented—he is both informative and persuasive. When he provides short summaries of other studies, the result is less satisfactory, and one has the feeling of swimming in descriptive conclusions too little related to any general principle.

Although the book's subtitle implies its focus as the implementation of new legislation, the discussion makes no distinction between these and other instances of program administration. Nor does it clearly separate types of programs or approaches likely to run into more or less implementation problems. Although the author endorses the new realism that argues that policy designers "should operate through manipulating prices and markets rather than through writing and enforcing regulations, through delivering cash rather than services, through communicating by means of smaller rather than larger units of social organization, and through seeking clearances from fewer rather than more levels of consultation and review," this is stated as an article of faith rather than as an implication of careful analysis. In the arena I know best—health care—the desire to maintain an illusion of market has resulted in layers of regulation and administration that far exceed what a more direct approach requires. I may be wrong in this observation, but at least the issue is arguable.

Bardach might have been more successful if he had depended to a larger extent on a series of analytic questions and less on a description of tactics. Under what conditions do social policies have more or less legitimacy, and how does this relate to speed of implementation? Under what conditions are groups or individuals in a stronger position to thwart social policies, and how might such power be neutralized? Are there differences in the implementation of technical as compared with value-related policies, between laws and administrative decisions, and between policies in the public and private sectors, and why? I am not sure these are necessarily the right questions, but if we are to further our understanding of the process we will increasingly need to focus our questions more sharply. Bardach's book introduces the reader to the importance of the problem. Implementation deserves a great deal of further study.

DAVID MECHANIC

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Decentralizing City Government: An Evaluation of the New York City District Manager Experiment. By Allen H. Barton, Norman I. Fainstein, Susan S. Fainstein, Nathalie S. Friedman, Stanley J. Heginbotham, Joel D. Koblentz, Theresa F. Rogers, John M. Boyle, and Ronald Brumback. (Lexington, Mass.: Lexington Books, D.C. Heath, 1977. Pp. 279. $19.00.)

During his last term Mayor John Lindsay established a program for decentralized administration of services provided by eight departments. The program involved an Office of Neighborhood Government (ONG) and 62 community planning districts, averaging a population of 130,000. Five of these districts were given a district manager in 1972 who worked with a cabinet composed of municipal service administrators (police, sanitation, parks, etc.) at the district level to improve service delivery. HEW provided operational funding for the program and NSF provided research support to Columbia University's Bureau of Applied Social Research to evaluate the experiment which is summarized in this book. Additional monographs, articles, and dissertations will appear on specific issues.

The Columbia study attempted to answer the question, "Will decentralization of big-city government help solve urban problems by improving service delivery to the public and by improving the public's relationships with city government?" The research design included four major components: administrative analysis, service delivery and cost analysis, a leadership study, and a public opinion survey.

At first, all five of the original district manager districts were part of the experiment, along with three control districts. However, one of the district manager districts was dropped from the survey portion of the research because of cost factors. Three of the other four districts with district managers were paired with comparable control districts in which no decentralization measures were taken. There was no appropriate district to pair with the fourth manager district, about 75 percent black, not poor, and with a large and active Jewish minority.

The selection of the districts was not random and even the objectives of the program were changed during the planning phase to deemphasize citizen participation and instead emphasize the administrative decentralization aspect. Further, the decentralization was not carried out as planned. The different department and administration leaders took different approaches. Police Commissioner Patrick Murphy already had decentralized and Human Resources Administrator Jule Sugarman found the program compatible with is own decentralization objectives. However, some of the other departments never delegated the necessary authority to their field units and several failed to change the boundaries of their subunits to conform to the district boundaries.

Therefore, the study would not qualify as either formal research or system evaluation. It was more of a demonstration project than an "experiment." It indicated once again the difficulty of performing social experimentation on a large scale with a moving target. The book does an excellent job of highlighting these difficulties and limitations.

The major findings were that departments having a history of line authority relationships with geographic units were in the best position to cooperate with the cabinets while, regardless of geography, the functional specialists, such as those in health, were less effective partners. The City Council and even the Mayor's office failed to give the districts the kind of cooperation and backing necessary to success, despite the fact that this was an administrative rather than a political decentralization. The demonstration essentially lasted two years. The study found no demonstrable increase in interdepartmental coordination as a result of the new program and dissatisfaction with service levels was not reduced. This was the perception of the field administrators as well as of the community leaders. Nevertheless, both groups felt very strongly that the program should be continued. The authors conclude that these participants saw great potential in the program and believed that if it had been given better support and had lasted longer these benefits would have been demonstrable.

This experience, when combined with the different participatory emphasis of the community action and model cities programs, suggests that some combination of administrative decentralization, central political support, and community participation is essential to the mobilization of the community in self-help as well as in the effective utilization of large scale governmental or private assistance. It was not reasonable to realize all the objectives of this program under the circumstances that prevailed. This approach and this case still have value, despite the presence of all the limitations of a case study.

Partly because of the inconclusiveness of the findings, the authors did little to relate the case to the general decentralization literature. The Urban Institute publications and the public choice literature are not even cited in reviewing the service delivery questions. The Urban Observatory research on citizen attitudes is not referred to in the citizen satisfaction material. Hopefully, forthcoming work will overcome this deficiency.

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The First Amendment and the Future of American Democracy. By Walter Berns. (New York: Basic Books, 1976. Pp. xi + 266. $12.50.)

In his new book, Walter Berns argues there is a need for a volume questioning the American expansion of freedom of expression as eloquent or persuasive as one of the half-dozen or so of its major defenses. That need persists.

It is Berns' argument (1) that the Supreme Court no longer follows the Founding Fathers' interpretation of the First Amendment, and (2) that their neologisms sap the very being of the republic. The first argument—which he develops at some painful length—is as surprising as would be an assertion that the Fifth Amendment (which overtly applies to crimes in which the penalty was loss of "life or limb") no longer bears its exact meaning. The second and more significant proposition is, in the main, asserted rather than argued and is virtually unsupported.

The volume follows a clear structural plan. The issues of freedom of religion, of political speech and of the press, and finally, the issue of obscenity, are discussed in terms of origins and Berns' self-enunciated rationale for each provision, followed by an account of Supreme Court errors. A surprisingly brief peroration attempts to cap off the argument.

By far the best part of the volume is the discussion of freedom of religion. This is a serious, well-reasoned, and even elegant argument. Berns' examination of Madison, Jefferson, et al., and the adoptive process suggests the Founders' assumption that freedom of religious doctrine was absolute and beyond governmental interest. The civil order has no interest in choosing between assertions that—Berns quotes Jefferson approvingly—break no bones nor rob any purses. On the other hand, the civil order is, he suggests, vitally concerned with the development of religious character as a buttress to republican order. The Founding Fathers, therefore, far from opposing governmental aid to religion, insisted—as in the Northwest Ordinance—on the civil uses of religion and the linkage, based upon purely nonreligious objectives.

Berns' effort is to paint the Founders as contemporaneously permissive on doctrine and expression in religion. "True, Tucker did defend the 'absolute and unrestrained exercise of our religious opinions' but so did everyone else. . ." (p. 114). But the persistence of blasphemy laws, as well as sectarian preference in individual states, shows how little of the modern spirit was infused into the law of the time. Berns concedes an illiberal limit to Christian sects in the Founders' attitudes, but a closer examination of reality will show their practice was so far from the needs of a pluralistic society that legal reconceptualizing was inevitable. Berns' argument about the Founders' view of personal character is historically convincing, but there is nothing in the constitutional text that legitimizes it. Nor does Berns advance anythong (other than the trappings of reverence) to suggest that the Founders had more compelling insight—let alone authority—regarding civic education than, say, pedagogy in general, the possibility of witchcraft, the principles of fine painting, or the need for a nuclear test-ban treaty.

The section on freedom of the press and speech—Berns continuously writes as if they were isomorphic—is confused and meandering. Purporting to quarrel with Levy's magesterial *Legacy of Suppression*, Berns seems only to place more emphasis on Hamilton's well-known argument in *People* v. *Croswell* than on a change of heart by the Jeffersonians. Levy suggested that learning from their mutual attempts to suppress each other, the federalists and anti-federalists developed tolerance for political expression. If I understand Berns, he suggests that the bulk of the Jeffersonians never become libertarians, remaining only states' righters. *Croswell's* linking of the right to publish only to "good motives" thus emerges for Berns as the basis of a constrained, rather than a wide-open tradition.

Like Levy, Berns makes no effort to explain why the draftsmen of the First Amendment tossed in a new freedom of speech if they meant only to restate common-law rights or what meaning a prohibition on prior restraint could have in conjunction with speech. But he has additional difficulties: the fact is that truth as an absolute defense had persisted as the respected rule in a minority of states even in the early years. And, of the handful of major state cases on libel and expression, quite a few—some as early as 1808—go well beyond that doctrine and embrace virtually unlimited privilege to comment on candidates.

In any event, Berns must and does acknowledge that the Founders' ideas evolved well past the original intent of the amendment. Apparently they were entitled to a learning experience but no one else is.

The chapter on obscenity further weakens any consistent argument on the nature of constitutional meaning that the book might have. In the absence of discussion by the Founders, Berns draws upon that great interpreter of American society—Rousseau. Even with such heroic borrowings, he can only fulminate against the Supreme Court, and suggests that a principled line can be drawn between trash and literature. But the Burger court, already floundering on just this issue, will not find here the shadow of the ghost of a hint of an idea on how to define such a standard.

The book growls to an end with a diatribe on the evils of modernity and the seductiveness of the trendy. This is supported at its climax—Berns apparently has no sense of incongruity—by a quotation from a column by Max Frankel in the *Times*. Unfortunately, Berns' talents are those of a polemicist or debater and his task calls for sterner stuff.

An author who finds that Holmes, Thomas Emerson, Felix Frankfurter, Hugo Black, and John Marshall Harlan all have identical views on the First Amendment minimizes his persuasiveness in suggesting that their analysis in small matters is inexact. One is even tempted to suggest that a plea for civility might exhibit a sense of respect for those who do not agree with each and every thought of the author. Does Berns not fear that his expressed contempt for Supreme Court justices will totally deprave his readers and ruin the last hope of republican virtue?

Three decades and more have passed since the Court allowed the Jehovah's Witnesses not to salute the flag. Mr. Berns has neither forgiven it nor spared it in this book, or his first. The republic has survived. It may be that other decisions of the Court have, in fact, weakened society, but having exhibited both mean-spiritness and disregard for evidence on causation in that instance, Berns' fulminations on broader questions loom ever more arbitrary. His cures and his reasoning are curiously reminiscent of Marcuse's, albeit the diseases and symptoms are different. But as Rousseau's well-known rival as expositer of the First Amendment, James Thurber, once said: It's just as bad to lean over too far backwards, as to fall on one's face.

SAMUEL KRISLOV

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Budgets and Bureaucrats: The Sources of Government Growth. Edited by Thomas E. Borcherding. (Durham, N.C.: Duke University Press, 1977. Pp. xvi + 291. $13.75.)

It is difficult to review adequately an edited work such as this one, which contains 14 chapters by 15 authors, although the contributors' like-mindedness gives this work greater coherence than is often found in the anthology form. With the exception of an essay by Lindsay and Norman, the book consists of chapters of varied quality by scholars associated with Virginia Polytechnic Institute and State University's Center for the Study of Public Choice. The authors are economists, although some, such as Tullock, have been contaminated by political science. That the epidemic has not spread farther is a major deficiency: several examples are cited below. Lindsay and Norman do display considerable wisdom from a political scientist's point of view, by observing, once again, that public sector inefficiency may or may not accord with one's ethical value system, and may or may not be regarded as a perversion of democracy. That efficiency is an appropriate objective overriding many others is the assumption/assertion of the contributors, a prejudice shared by the reviewer.

The authors confront a profoundly important question: why has government expanded so greatly in this century? The dimensions of growth from 1870 to 1970 are shown admirably by Borcherding (Ch. 2), whose 22 tables are a rich data source on the question. For many reasons, the writers consider some portion of this growth to have resulted from structural imperfections in the polity. (The redistributional aspect of growth is largely ignored.) Buchanan (Ch. 1) asks: what portion of this growth is due to democratic processes and what portion, given his conception of responsive government, can be regarded as excessive? Borcherding's analysis (Ch. 3) is illuminating. He attributes (roughly) 20 percent of public sector growth in this century to population increases, 25 percent to greater affluence, 10 percent to increased costs and relatively inelastic demand for government services, 4–5 percent (at the most) to federal aid tied to (inducing) state and local expenditures. Interestingly, urbanization as such has had no apparent impact. Excepting federal aid, these increases result from "nonpolitical" or more or less ideologically neutral processes. But 40–50 percent of growth is left unexplained by such factors. Potential or actual causes are enumerated. Director's Law of Public Expenditures, first explicated by Stigler, is a possibility: increasingly, expenditures have benefited the middle classes, whose numbers include the median voter, and who have managed to displace related costs onto the poor and the rich. (Why the median voter has not yet expropriated all wealth from the rich is not explained.) Other possibilities: fiscal illusions (although Borcherding is skeptical about this, but see Goetz, Ch. 10), the proliferation of interest groups, the absence of competition (unfortunately, the vast and important literature on political competition and spending in the American states and local jurisdictions is not cited) and monopolistic bureaucracies. In connection with the last point, Spann (Ch. 6) and Orzechowski (Ch. 13) show that public sector productivity is less than in private service and retailing sectors, and argue that the inefficiencies of public agencies are responsible. Spann's comparative study (Ch. 4) of the costs of public and private entities delivering similar services bears on the same question. Airlines, garbage collection, hospitals, fire protection and electrical utilities can be (have been) provided more efficiently by private firms. Whether politically competitive public jurisdictions are more or less efficient is not examined, on the grounds that "empirical estimates of the degree of competition between political parties are hard to come by" (pp. 72–73). This is not true. The Ranney/Kendall (and Ranney's refinement), Dawson/Robinson, and Hofferbert indices (and others) are readily available.

The impact of voting bureaucrats on public sector expansion is considered by Bush and Denzau (Ch. 5). Their conclusion is largely conjectural and based empirically on old and limited data: bureaucrats have more than proportionate political power because of their high propensity to vote (indeed, perhaps highest of all "minority" groups), and they typically have an interest in enlarging the public sector. Linkages between voting and policy are presumed, even though the empirical studies of voting preferences and public decisions are most equivocal on the subject, if not downright negative. In small jurisdictions with single-issue voting (e.g., bond issues, school budgets) the proposition is more reasonable. Huge increases in public school expenditures, Staff argues (Ch. 7–8), largely benefit teachers and administrators and have not improved student performance (watch *Serrano* for evidence, he suggests).

A formal model of public and private sector growth is developed by Bush and Mackay (Ch. 11), which merits empirical analysis. Borcherding, Bush and Spann (Ch. 12) consider quasi-formally the influence of public outputs (whether public or divisible), bureaucratic activism, and population congestion on public spending—interesting, but again testing is needed, as the authors acknowledge.

What is to be done about it all? Tullock (Ch. 15) suggests further study, an agreeable note on which to end a commendable and stimulating book. In this connection, two observations seem in order. (1) The budget-maximizing bureaucrat is a plausible creation, but the budget-minimizing bureaucrat (or politician) is not unknown either: the role of the latter in the scheme of things might also merit study—that's what makes politics, after all. (2) Economists working in public choice often display unawareness of literature, data files, theoretical constructs and operational definitions in political and other social sciences that are directly relevant to their work. This may be forgiven in new ventures, perhaps, but, if it persists, the charge of shabby scholarship will inevitably discredit the public choice movement.

On balance, then, this is a good book. It deals with an important subject, presents reasoned arguments, useful findings and suggestive hypotheses.

The index is mystifying. As an example, J. K. Galbraith, whose views are moderately important to one essay, and Ronald Reagan, whose views are not, are indexed, while William Niskanen and W. J. Baumol, whose themes are central to several chapters, are omitted.

L. L. WADE

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John F. Kennedy and the Second Reconstruction. By Carl M. Brauer. (New York: Columbia University Press, 1977. Pp. xi + 396. $14.95.)

The brief presidency of John F. Kennedy continues to attract proportionately more attention from scholars and journalists than other recent administrations. The principal value of this new addition to the burgeoning literature is in its extensive use of primary sources, including recently opened papers at the Kennedy Library, to document the chronology of civil rights events during the Kennedy years.

This volume, one of ten that have been published in the Contemporary American History Series since 1969, fulfills the author's stated purpose to describe "Kennedy's role in promoting this modern attempt to remove racial barriers to equal opportunity in American life" (p. ix). In recounting the evolution of Kennedy's views on civil rights, the administration's response to the racial crises in the South in the early 1960s, and the struggle later in the administration for legislation, Bruaer weaves an important story with great attention to detail. However, for the experienced scholar of the presidency or of civil rights, the volume adds nothing startling, nor does it break new ground in the interpretation of these events.

The author's main contention that the initiatives of the Kennedy administration constituted the critical first stage of a Second Reconstruction is not defended against possible alternative interpretations. Might not Truman's initiatives have been the "critical first stage?" Brauer does suggest that Harry S. Truman pioneered in the employment of presidential power on behalf of racial progress, but that Kennedy, by contrast, "turned Truman's trails into wide avenues" (p. 315). While he argues that civil rights became a focal point of public policy and political debates under Kennedy, he does not address the charges of the critics of Kennedy's record on civil rights (such as Miroff, Navasky, and Zinn) who contend that Kennedy did not take the initiative on civil rights, but claimed credits for forward steps in a movement directed by others. One wishes for more interpretation of the data accumulated so meticulously, as well as explicit attention to the methods used to determine "Kennedy's motives" and "the critical first stage of the Second Reconstruction."

Furthermore, the value of Brauer's thorough investigation of primary sources is diminished in usefulness to the serious scholar by his footnote style. The notes corresponding to the number at the end of almost every paragraph include from one to eleven citations. Often it is unclear as to whether all of the citations support one point or apply to different sentences, and if the latter, to which ones.

RUTH MORGAN

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The Dynamics of Party Support: Cohort-Analyzing Party Identification. By Philip E. Converse. (Beverly Hills, Calif.: Sage Library of Social Research, Vol. 35, 1976. Pp. 175. $11.00, cloth; $6.00, paper.)

Reading this book virtually puts one in a small seminar in which Converse shares his personal insights on the relationship between birth cohorts and party identification. His use of the first person gives the reader a sense of intimacy and participation in the process of discovery. The material, however, is quite technical and oriented toward specialists in voting behavior; and the going is occasionally slow as Converse rambles through the finer points of cohort analysis.

Converse's major objective is to make a definitive statement on how age, generation, and history have combined to influence the distribution of partisan support in the United States during the past 30 years or so. His data base is a unique file of tabulations "drawn progressively from twenty-nine national samples of the adult population conducted by the [University of Michigan's] Survey Research Center between 1952 and 1975" (p. 161). The data are not maintained as individual-level records; rather, Converse has aggregated the responses to the SRC party identification scale by exact age and birth year. Tabulations are prepared by combining these aggregates in an appropriate fashion. The utility of this data set is amply demonstrated throughout the book; but, as Converse himself admits, the process of aggregation has precluded the possibility of using any control variables, such as race, sex, and region. This shortcoming is a distinct limitation which leaves many opportunities for further research by any scholar with sufficient patience and an unlimited computer account.

Converse begins his presentation by defining life-cycle, generation, and period effects and by discussing the methodological impossibility of dissentangling all three. He hurdles this roadblock by invoking "side information" and the criterion of "plausibility." Cohort analysis is a very tricky business, and Converse does not hesitate to accept help from any available source. Other scholars may quibble with his choice of side information and the plausibility of his conclusions, but Converse consistently presents well-reasoned arguments.

From here, Converse launches a major analysis of the strength component of party identification. Unlike other researchers in this area, his first step is to examine a plot of mean party identification strength over time. His graph clearly shows two distinct periods. Between

1944 and 1964, the means fluctuate modestly about a horizontal line. Converse terms this the "steady-state" period. From 1965 through 1975, however, the graph shows a continuing decline in mean strength. To avoid confounding the different effects which produced these distinctly different trends, Converse proceeds to analyze each period separately.

During the steady-state period, Converse concludes that life-cycle gains alone account for the static age-strength relationship. As people aged, they had a tendency to report stronger partisan attachments. The overall steady-state was maintained by young, weakly attached persons entering adulthood to replace the older, strongly attached persons who were dying.

This conclusion contrasts sharply with the results reported by other researchers, principally Norval Glenn and Paul Abramson. The disagreement lies in the procedures employed to test life-cycle and generational effects. Earlier researchers have tended to compare only the values at the end-points of the period covered by their data. Between sampling error and the confounding of the two distinct periods, these researchers have been unable to locate a life-cycle effect. Converse argues that one should utilize all the data in between before judging the case. He does this by computing regression slopes for each cohort and finding that ten of the thirteen do show positive values as would be expected if a life-cycle effect predominated. When the same procedure is applied to chronological age groups, no generation effect is found.

Converse's analysis of the post-1964 period finds that period effects predominated. The general decline in strength was characteristic of all cohorts due to the influence of historical factors operating during this period. These historical factors overwhelmed the aging effects found in the steady-state period, and the overall decline was stronger than could be explained by the entry of new generations with low levels of partisanship. Although he is unable to provide a firm explanation for the period effect, Converse speculates that it is the result of the joint impact of the civil rights movement and the Vietnam War.

Having completed his analysis of partisan strength, Converse quickly presents the evidence concerning the other component of party identification—the direction of partisan preference. Without any difficulty he shows that direction is primarily the result of generation effects, a conclusion which agrees with most of the recent literature on this subject. Interestingly enough, Converse is able to document the impact of period effects upon the original generational imprinting. By dividing his data set into the early (1952–1962) and later (1963–1972) surveys, one can see the influence of additional historical experience upon the cohorts. The early data show clear breaks between the Republican transition, and Democratic socialization periods. In the later batch of surveys, the breaks are still evident, but they are fuzzier for the older cohorts.

Although Converse's analyses are skillfully executed, there are some flaws and limitations. To his credit, Converse himself generally admits to them. The problem of the aggregate nature of his data set has already been mentioned. His data also combine voters with nonvoters. Given the volatile preferences of those persons who are too apathetic to vote, the analysis could be strengthened at several points by omitting the nonvoters. In addition, Converse sidesteps the issue of short-term influences due to a pending election and the differences between presidential and congressional election years. At times he is also sloppy in labeling figures and tables and in explaining their content. A cohort matrix or two would make it easier for the reader to get a feel for the data that underlie the discussion.

All in all, this book is a major contribution to the study of party identification. It is not a text on how to do cohort analysis, but it is an excellent example of the careful detective work that social scientists must do when studying the complex phenomena which characterize our discipline.

WILLIAM R. KLECKA

University of Cincinnati

Black Elected Officials: A Study of Black Americans Holding Government Office. By James E. Conyers and Walter L. Wallace. (New York: Russell Sage Foundation, 1976. Pp. ix + 190. $8.95.)

This study is one of two currently dealing with the emerging phenomenon of black elected officials (the other is *Blacks in Power* by Leonard Cole, Princeton, 1976). James E. Conyers and Walter L. Wallace should be commended for their timely and long-awaited book on this subject.

The quantitative data of the study, collected in 1971, are based upon a 43 percent return (799) from the 1,860 known black elected officials at all levels of government except the federal. Another, smaller, sample was drawn from white elected officials (484) in order to compare the two groups. The researchers were concerned with attitudinal and contextual milieus of both black and white respondents, each of whom was sent an extensive interview schedule probing broad issue areas. Among the areas examined are the political expectations, beliefs, motivations, personal backgrounds, and party affiliations of the leaders. In addition, campaign contributors and the electoral impact of black politicians are analyzed. The intent of this work is not only to analyze the attitudes of black and white leaders, but most importantly to achieve a deeper understanding of black political leadership.

To attempt an enterprise of this magnitude is courageous, but the study leaves some questions unanswered, others unexplored. One of the major difficulties is the use of appropriate language. Many of the terms are either misleading or inadequately explained. There is also little regard for social science literature concerning the black politician. Few studies deal with this topic, but published materials should have been cited. The researchers should have reconciled their findings with the voluminous literature on political leadership. Regarding political beliefs and motivations of black elected officials for example, it would be desirable to cite some earlier studies, particularly those by James Q. Wilson, Harold Gosnell and Thomas P. Monahan. Also useful are political recruitment studies published by Kenneth Prewitt, focusing on elected officials generally, and the political biographies of James D. Barber and others. This literature is completely ignored.

Conyers and Wallace appear to delight in using nontraditional terms which do little to help but a great deal to confuse the presentation of data. They also seem to have difficulty in assessing the implications of specific findings. For example, they asked their respondents to register degrees of agreement and disagreement with the following statement: "Inherited racial characteristics play more of a part in the achievement of individuals and groups than is generally thought." It is then argued that a "strongly agree" response to this question qualifies one as a "hereditarian" which they insist is a preferable scientific term to the more traditional term "racist." Many objections can be made to this conclusion. First, current methodological conventions cast serious doubt on the wisdom of inferring an attitudinal predisposition from one Likert scale item. Second, even were one item sufficient, "hereditarianism" as an attitudinal trait cannot be inferred from the specific item presented for inspection. Third, even if the item were a valid indicator of "hereditarianism," that concept should not be equated with the extreme dimensions of "racism." Similarly, using the word black "independence" as a substitute for black "separation" which the authors say they like better, is not necessarily a help to the reader. What does black "independence" mean? From the questions used to generate this scale item, one is not sure whether they are confusing individual preferences for personal autonomy with the goals of a political position.

Finally, the Conyers and Wallace book is an example of scholarly malnutrition. In their index of references, there are only 31 citations mentioned and almost two-thirds of them were published before 1970, and just 6 citations were printed since 1971; the growing literature published since them is ignored.

One is left with the regret that this effort produced such a modest result. The authors hoped that this study would stimulate further explorations, and one can accept their effort as a tentative beginning. A much more definitive study of black elected officials is needed, however, before the full attitudinal and behavioral impact of these officials on the American political system can be assessed.

JAMES LEE ROBINSON

Rutgers University

Political Reform and the American Experiment. By William J. Crotty. (New York: Thomas Y. Crowell, 1977. Pp. xiv + 312. Paper, price not listed.)

Over the past decade many changes have occurred in the structure and operation of many of the major political institutions in the United States. Both houses of Congress, the office of the president, the electoral process, and the political parties have all fallen victim. Some of these changes were major; others were minor. Some were the result of natural political evolution; others were the product of deliberate political engineering. Whatever the cause and degree, it is safe to say that changing the rules changes the game, and that keeping abreast of these many recent developments and speculating on their probable political consequences will consume the time and energy of many students of American politics.

William J. Crotty contributes to this small but growing literature, first, by compiling into one volume recent reform efforts in three important areas: suffrage, campaign finance, and presidential nominations; second, by tracing historical developments in each of these areas, thereby providing a perspective from which recent reforms can be viewed; third, by evaluating these reforms and other alternatives in light of which political actors and societal values win and lose; and fourth, by developing from the country's past and present experience with political reform a reform model that helps explain the periodic emergence of political reform and counter-reform movements which often follow. All of this is done, as the author forthrightly acknowledges—and as readers should be forewarned—"from a somewhat more activist orientation than one would normally expect from a political scientist" (p. xi).

Part I of Crotty's book is devoted to voting. He begins by tracing the evolution of the franchise from colonial times, when suffrage was restricted to white males of good character and economic substantiality, to the present, where despite the removal of many of these barriers, turnout still remains lower than in other Western democracies. Along the way, the reader becomes reacquainted with literacy tests, grandfather clauses, residency requirements, suffragettes, voting rights acts, registration laws, and such empirical and normative questions as who does and does not vote, who should hold the franchise, and how difficult it should be to qualify to vote. Crotty closes Part I with an evaluation of some of the alternative remedies for voter apathy. Included in his analyses are public relations programs, standardized voter qualifications, a national voting commission, universal voter enrollment plans, postcard registration, and methods for simplifying absentee voting.

Part II of *Political Reform and the American Experiment* deals with the problems of campaign financing and these chapters alone are well worth the price of the book. Here Crotty documents the spiraling cost of electoral politics at all levels of government, discusses the advantages unrestrained financing bestows upon certain types of candidates, interests, and contributors, and recounts past and present reform efforts aimed at regulating political spending. Particularly illuminating is the section on Watergate. Thanks both to the determined investigators who uncovered the improprieties and illegalities, and to Crotty, who sifted through the written record and assembled an assortment of intriguing examples, the reader learns the fine art, as practiced by the Nixon administration, of extracting contributions, laundering money, repaying contributors, selling ambassadorships, disguising donations, and perverting governmental agencies. The examples in some instances assume the characteristics of a mystery thriller, but there is nothing entertaining or uplifting about them. Although Watergate is now a tragic part of our nation's history, it has left a redeeming legacy. Watergate has awakened the nation, albeit with the force and shock of a sledgehammer, to the many problems of money and politics and provided the catalyst for much-needed reform.

Unfortunately, as the readers of Crotty's book will soon discover, the problems are still far from solved. The reforms enacted in response to Watergate became the target of a counter-reform movement, whose strategies and tactics ranged from testing the constitutionality of the new laws in federal courts to limiting the independence of the Federal Elections Commission by placing all commissions actions and regulations under Congressional scrutiny. Crotty uses this behavior and reaction to the post-Watergate reforms as the appropriate illustration of the uncertainty, recrimination, and eventual reformulation that he contends marks most post-reform stages.

Presidential nominations and party reforms share the limelight in Part III. The gradual trend toward structural democratization of the presidential nomination process is traced from the days of King Caucus to the present multitiered process of primaries, caucuses, and conventions. In the area of party reform, the author reviews the actions, recommendations, and implementations of the Hughes Commission, McGovern-Fraser Commission, Sanford Commission, and Milkuski Commission in the Democratic Party, and the DO Committee and Rule 29 Committee in the Republican party. Just as in Part I and Part II, Crotty closes with his own evaluations of many of these reforms and offers his own recommendations based on what he perceives to be "workable, democratic in intent, and realizable" (p. xi).

JAMES I. LENGLE

Georgetown University

One Nation, So Many Governments. By Michael N. Danielson, Alan M. Hershey and John M. Bayne. (Lexington, Massachusetts: Lexington Books, D.C. Heath, 1977. Pp. xii + 141. $14.00.)

In 1972 the Ford Foundation began to review several of its major program areas including state and local government, especially the consequences of their rapid growth and the changes in their role in society's efforts to cope with poverty, inequity, and inefficiency. In particular, the foundation wanted an examination of the impact of reapportionment, the increase of functional autonomy, developments in metropolitan structures, the role of public employee unions, and the status of efficiency in service delivery. Michael Danielson directed the foundation's project on state and local government; part of that report formed the basis for *One Nation: So Many Governments.*

The authors adhere closely to Ford's concerns. In the first three chapters they "set the stage" by describing urban social and political developments with emphasis on recent issues such as fear of crime, fading commitment to racial equality, ebbing institutional legitimacy, etc. No 56-page review of recent urban history, including this one, can do full justice to the subject, but it introduces major topics and mentions major trends. The primary weakness in this first section is the implication that past social, economic, and political trends will persist and continue to serve as the basis for succeeding rounds of state and local problems. There really should be more indication of what the implications of the "energy crisis" might be for state and local government or the consequences of the continuation of the post-1970 outflow of population from SMSA's.

The heart of the report comes in the fourth chapter describing changes in state and local institutions and, to a lesser degree, in the fifth chapter analyzing state and local government finances. Here the authors focus on reform: of political machines, of inter- and intrastate metropolitan institutions, of functional autonomy, of powerful public employee organizations, of weak government executives, and of state and local legislative malapportionment.

Like the earlier chapters, these two are good summary statements of the basic ebbs and flows in state and local governmental institutions in the twentieth century, but here the authors have a second message as well. They remind their audience (the Ford Foundation at the very least) that, at best, reform is a perilous business because reform issues always have at least two sides; that reform often benefits those already holding power; and that somehow reform usually misses what Danielson et al. regard as the most serious and persistent issue facing the political system—inequity in the society.

This last point illustrates how the authors are transformed occasionally from social scientists analyzing, at the behest of their patrons, the problems of and prospects for reform, into reformers in their own right. Throughout the report the decentralized American system and the major changes in state and local governmental institutions are evaluated, and often found wanting, because of their limited capacity to "promote equity, the long-proclaimed goal of equal access to opportunities for occupational, residential, and recreational advantages, as well as equal access to legal justice and the protection of the law" (p. 14).

Ultimately, the authors conclude that strengthened governmental institutions cannot be the sole end of reform; "institutional reform should not be preferred merely because it is easier to formulate or achieve than objectives such as greater equity and enhanced opportunity for the poor" (p. 96). The authors tell us that we have paid a price for our decentralized political system which "promotes political participation, facilitates conflict resolution, and reduces the need for large centralized bureaucracies" and the price is a "system which seriously limits the comprehensive airing of public issues" (p. 130).

Danielson et al. give these themes a thorough discussion and in the process have produced a thoughtful report to the Ford Foundation as well as an interesting and useful supporting text for courses on federalism and intergovernmental relations.

The problem for some readers may be whether or not the indictment of the decentralized political system is perhaps premature and, indeed, unfair, given all the other demands we make on our state and local institutions. Students, certainly, will be inclined to raise such questions.

As for myself, a midwesterner, I am disturbed that these three eastern authors not only draw most of their examples from eastern seaboard locales but also failed to detect a major error when they referred, on page 61, to the Prendergast (sic) machine.

THOMAS M. SCOTT

University of Minnesota

Bureaucracy, Innovation, and Public Policy. By George W. Downs, Jr. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xiii + 150. $14.00.)

George Downs has conducted a very careful and competent analysis of the deinstitutionalization movement in juvenile corrections. His aim is to combine the fine detail usually found in a bureaucratic case study with the systematic comparison typical of a quantitative policy output study. The cross-sectional data base consists of qualitative and quantitative information gathered from exploratory visits to 16 states, from structured interviews with corrections officials in all 50 states, and from statistical records in all 50 states. The author is thoroughly familiar with the policy determinants literature and with the diffusion-of-innovation literature in sociology, economics, and political science. He uses the previous research to great advantage to generate hypotheses for variation in deinstitutionalization among the states.

The focus of the work is fairly narrow; a single dependent variable is explored throughout the book. The measure of the innovation is the percent of juvenile offenders in community-based residential facilities. This is a measure of the degree or extent of innovation rather than the relative speed of its adoption. It is quite an appropriate measure and indeed an improvement over the usual date of adoption measures. However, I disagree with the author's claim (p. 41) that the deinstitutionalization rate is not an expenditure measure. Community corrections facilities involve start-up costs and operating costs. Furthermore, the prisons which have relatively fixed operating costs usually remain open for violent offenders. Therefore, the short-run total expenditure figure for incarceration most likely goes up.

The author organizes his investigation into three parts: the impact of socioeconomic variables, the impact of the task environment, and the impact of bureaucratic and executive variables upon the rate of deinstitutionalization. He makes much of his finding that traditional economic variables have little relationship with the dependent variable. I suspect this lack of association occurs because federal funds from the Law Enforcement Assistance Administration may have financed quite a few of the community corrections facilities, thereby rendering the level of state wealth less important. Another interesting finding is the somewhat stronger (and positive) effect of social homogeneity upon the deinstitutionalization rate. Downs is able to show that the functional relationship between homogeneity and the innovation is that of a resource constraint, i.e., homogeneity is a necessary but not sufficient condition for innovation.

The main theoretical lesson of the section on the impact of the task environment is that although socioeconomic variables are not very important, neither are most political ones. A further policy lesson is drawn from the finding that both interest group activity and the number of administrative reorganizations increase the rate of deinstitutionalization. Downs suggests that before reform can take place, corrections must be a highly visible issue in a volatile atmosphere.

The final section similarly contains findings which are significant in both a theoretical and a prescriptive sense. Contrary to theoretical expectations, Downs finds that organizational complexity and centralization are not important determinants of the rate of innovation. The most important factor, and a manipulable one, is the presence of an ideologically committed director in an autonomous agency.

This book will interest a wide variety of readers as it is a model of what comparative state policy research should be like. It is theoretically oriented; it enlightens us about the substance of the one policy studied; it moves away from the politics-versus-economics controversy into the study of a neglected but more important actor: the bureaucracy. Hence, students of policy determination, innovation, criminal justice, the bureaucracy, and state politics might learn from reading this short book.

VIRGINIA GRAY

University of Minnesota

National Politics and Sex Discrimination in Education. By Andrew Fishel and Janice Pottker. (Lexington, Mass.: Lexington Books, D.C. Heath, 1977. Pp. ix + 159. $15.00.)

Among areas of concern to feminists none has received more official attention than education. The reasons are obvious. First, girls make up over half the primary- and secondary-school student population in the United States, and women are a substantial minority of students in higher education. Second, for historical reasons and perhaps *faute de mieux*, a disproportionate percentage of all American professional women have gravitated to education; in general, these women have been schooled through or past the undergraduate level, they are articulate, and they know that their being female vitally affects their professional advancement and fulfillment. Third, in a society that takes universal public education for granted, government plays an extensive, direct and ideologically accepted part in shaping educational policy and practice.

The book under review successfully presents a panorama of national politics and sex discrimination in education by concentrating on four important cases. It provides a contextual framework for them in brief but accurate surveys of relevant aspects of the women's movement in the last decade and of the present situation of women in education. The cases themselves have been selected to illustrate judicial, legislative and bureaucratic-administrative processes of dealing with or finessing sex-discrimination issues and demands for identification, study and resolution of salient problems in the area. A chapter contributed by two participants in the establishment and operation of the U.S. Office of Education's Task Force on Women's Education vividly conveys the sense of the ways of bureaucratic politics.

Given the brevity of the work, the individual cases cannot be sufficiently documented or their presentation sufficiently detailed to satisfy requirements of definitive scholarship. They are, however, adequate introductions to the kinds of situations encountered and to current means of managing them. They also support the well-taken and politically sound analytical chapter summaries and the final chapter, which draws more general conclusions and points out prime areas for continued and future study and action.

The book will be useful for the general reader and for students in undergraduate courses in the American political process and in women and politics. In such courses the case chapters will lend specificity and a sense of reality to more generalized textual and lecture material. Few such case collections are now easily available. For all its current prominence, sex discrimination in education has attracted public and official attention only in the last few years. Such case studies as have been made are scattered in many journals and other publications, or are unpublished; few of them are broadly accessible. Four good, if brief and general, case descriptions in one volume will be a useful convenience to students, instructors and others. In terms of substance, this book is a good supplement, for example, to surveys such as Irene Murphy's excellent *Public Policy on the Status of Women* (Lexington Books, 1973), which necessarily treat particulars summarily.

Having given substance its due, one must join the growing number of *American Political Science Review* reviewers who protest stylistic and even grammatical and orthographic carelessness (or worse) displayed by authors and editors. Was "$200,000 million" (n.b.: $200 billion) *ever* "authorized to be spent on [a] Special Projects Act" (p. 81), or, for that matter, on all federal categories for education combined, in a given appropriation cycle? What is a "barage [sic] of letters" (*idem*)? Such lapses annoy the reader. Others are more serious; at several places they result in ambiguity or meaninglessness. In a short review these cannot be explicated, but they will manifest themselves to any reasonably careful reader. Praise for this work must be tempered with a warning that it will reinforce the current disrespect for language and clarity currently so rife among students and, alas, many of their teachers and mentors as well. Given the authors' findings, one is tempted to suggest that women in education have enough trouble without having their problems discussed in a style of tin-ear illiteracy.

Notwithstanding this serious weakness, the book addresses a significant concern, and offers the reader some lessons applicable to women's or educational issues in the public forum, and to all public policy. One of these is apparent in the clear implication that, under whatever disguises, virtual representation is ineffective in protecting the interests of the virtually represented. Another is that, for major social change to be seriously contemplated and supported by legislators and officials, they must sense it to be urgent and connected with both society's needs and their own exigencies and goals: "We respond to social turmoil" (an official of the Office of Civil Rights, quoted at p. 4). For women to achieve genuine equality of opportunity with men in the various sectors of education would be a major social change in the United States. If women, in education and elsewhere, elect not to use the weapon of social turmoil (which is not to be confused with the annoyance and sense of attack felt by any privileged group when outsiders demand admittance), they must devise other means of getting serious political attention. Perhaps tact, shrillness, abrasiveness, persistence, and votes, can be made to work.

COREY VENNING

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Power and Politics in the United States. By G. David Garson. (Lexington, Mass.: D.C. Heath, 1977. Pp. viii + 352. $4.95, paper.)

G. David Garson's *Power and Politics in the United States* is a book that is difficult to categorize and thus difficult to evaluate. Garson begins by claiming that he has written a primer suitable for introductory courses in American politics or political sociology (p. vii). Moreover, the author notes that he will explicate the stuff of American politics using a "political-economic framework." Given the common strategy of using some loose analytic framework to integrate materials in American politics, this claim causes no surprises. If one is hoping for the familiar simple-minded parade of Introduction to American Government topics, however, one will wait in vain.

One realizes that this is not the promised introductory book right from the beginning when Garson launches a discussion on the meaning of political power. Among other things, Garson distinguishes between authority, influence, and affinity, describes the emergence of "non-decisions" as an important concept, and tells us of Edelman's notion of symbolic politics (to mention just a few topics covered). The cataloguing of power-related concepts continues into chapters 2 and 3 where we are given a fairly detailed theoretical and historical treatment of elite and pluralistic arguments together with the familiar discussions of how should one measure power (reputational method, decision method, sociology of leadership, etc.).

For those students who have survived all the theoretical and methodological debates, the substantive materials appear in chapters 4–7 where the various approaches to analyzing power are concretely illustrated. Garson uses the structural approach to show the operations of military power, he explains the Vietnamese War by the decisional approach, and illustrates the reputational and distributional approaches with a wide variety of sociological and economic data. Chapters 8–11 present an eclectic history of American politics with a heavy emphasis on economic factors, especially economic conflicts. One must assume that this is the making good of the much earlier promise of the "political-economic framework." The book is wrapped up with a short conclusion in which the "political-economic framework" is asserted to be a preferred alternative to either pluralist or elitist theory as a way of understanding American politics.

What exactly is this new, improved product? Unfortunately, other than being told that politics and economics are closely interrelated, we are offered little in the way of a new or precise analytical framework. The closest thing we get are nine "Starting Points in Political Economy" which reiterate in different ways the simple observation that politics and economics go hand in hand. It is unclear whether these starting points are testable hypotheses, necessary analytical assumptions, or self-evident conclusions in need of further fleshing out.

How is a book like this to be judged? Is it a poor text because of its emphasis on abstractions and methods or is it a poor scholarly book owing to its inclusion of substantial amounts of "obvious" descriptive material? I suspect that most students interested in simplistic accounts of American politics will find the book largely baffling, while scholars looking for elegant integrations of theory and data will judge Garson's efforts as inadequate. At best it provides a few general hypotheses and a loose analytical framework that may prove useful for someone with little background in the field.

Such a prediction is not, however, a negative assessment. As a useful non-simple minded introduction to a wide variety of topics relating to power and economic-political relationships, this is a fine book. *Power and Politics in the United States* is well written; it is filled with a great deal of interesting and (for me) new information; and it is occasionally insightful. Garson's command of history, economics, and political theory is quite impressive. If one is interested in the diverse topics covered by Garson—and these are too numerous to list—this book offers about as good an introduction as one is likely to find.

One cannot leave *Power and Politics in the United States*, however, without wondering about how that book came into existence. Was Garson really trying to reach freshmen or was the little introductory claim something to keep the publisher happy? Or perhaps this was a grand project at integrating economics and politics that fell short and thus became a text. My own feeling is that Garson wanted to write a major treatise on political and economic power, but because of the nature of the contemporary publishing world, he had to dilute it for the sake of the market. Perhaps his next book should be about the economics of political analysis.

ROBERT WEISSBERG

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The Ages of American Law. By Grant Gilmore. (New Haven and London: Yale University Press, 1977. Pp. x + 154. $10.00.)

As recently as 1972 American legal history could accurately be described as rather neglected. This is no longer the case. The flood of publications in this field is indeed one of the most remarkable developments of the last five years. *The Ages of American Law,* based upon the Storrs Lectures of 1974, is a welcome addition to this literary deluge. The author is a scholar of impeccable credentials who is Sterling Professor of Law at Yale. Although he disclaims any intent to contribute to the scholarly literature, his little book is a valuable supplement to it.

Grant Gilmore's express purposes are to describe the course of American law since 1800 and to speculate on why it developed as it did. The focus of his book is legal doctrine rather than institutions, legislation, or administrative decisions. Most of the author's illustrations are taken from commercial law, his particular field of expertise. At any rate, an unstated purpose of this informed and pungently written book may well be to provoke controversy and outrage. If this is one of his aims, his work is an unqualified success in achieving it.

Controversial judgments abound in *The Ages of American Law,* including the introductory chapter. Gilmore's justification of his disregard of colonial legal history is a good example. Published reports of judicial decisions were not regularly available until 1800. Before this date, therefore, "nothing which could rationally be called a legal system" existed (p. 9). The implication of this dubious argument for the law of primitive societies, and some highly developed ones, is obvious and unjustifiable. Nonetheless, the author divides American legal history into the three stages outlined by the late Karl N. Llewellyn.

The focus of the second chapter is the period from 1800 until the Civil War, the "Age of Discovery." Gilmore's analysis of the various attempts to unify American law is particularly valuable. They include codification, the authoritative formulation of legal doctrine in learned treatises, and the decisions of the Supreme Court, the most important of which is *Swift* v. *Tyson* (1842). Mr. Justice Story's opinion in this landmark case "accurately reflected the ... innovative spirit which was a noteworthy feature of American law during the pre-Civil War period" (pp. 34–35).

The subject of the third chapter is the era from the Civil War to World War I, the "Age of Faith." Gilmore discusses in detail the contributions of C. C. Langdell and O. W. Holmes, Jr., who are the "twin symbols of the new age" (p. 56). From his pivotal position as Dean of the Harvard Law School Langdell had a profound effect on legal education in America. He believed that law is a science, with a few unchanging legal doctrines as its basis. These doctrines are best understood by careful study of the small number of properly decided reported cases. The others, the vast majority of the whole, are worse than useless for the purpose of scientific study. Gilmore has little use for Langdell's "crude and simplistic" ideas (p. 56) or their "essentially stupid" author (p. 42). The great "accomplishment" of Holmes as a legal scholar was to make "Langdellianism intellectually respectable" (p. 56). The author of *The Common Law* (1881) developed a unitary theory "which would explain all conceivable single instances and thus make it unnecessary to look with any particularity at what was actually going on in the real world" (p. 56).

The time-span from World War I to the present is the focus of the fourth chapter. The most provocative sections of this part of the book are the author's interpretations of Cardozo and the legal realists. Cardozo is alleged to be the perfect symbol of the "Age of Anxiety," whose best-known book has "almost no intellectual content" (p. 76). Only the tenacity of Langdellianism explains the furor which *The Nature of the Judicial Process* aroused. The impact of this nineteenth-century dogma is even apparent in the work of the legal realists. They did not in fact abandon Langdell's most basic premises, such as the belief in a science of law and "the one true rule of law." Rather, these juristic rebels merely proposed new means of developing the science and of discovering the rule. The proposals seem to me, however, to reflect *concepts* of law and of legal science which are entirely different from Langdell's. To that extent, Gilmore's interpretation of the realist movement obscures its distinctiveness.

In the final chapter the author criticizes the "illusion" of a science of law, the aim of which is prediction. He evaluates the principle of the Rule of Law, as well as the New Conceptualism of some legal scholars. He attempts to explain the explosion of interest in legal history as a response to the crisis of Western thought. He concludes by emphasizing the need "to keep our theories open-ended, our assumptions tentative, our reactions flexible" (p. 110).

In sum, the scope of *The Ages of American Law* is quite narrow and some of the judgments of the author are outrageous. Nevertheless, the book is informative, unusually well written, and lively. It is therefore recommended for any political scientist with an interest in American legal thought or history.

WILFRID E. RUMBLE

Vassar College

Democratic Promise: The Populist Movement in America. By Lawrence Goodwyn. (New York: Oxford University Press, 1976. Pp. xxvii + 718. $19.95.)

Populism, the most significant assault on industrial capitalism in American history, was discredited by some political opponents and domesticated by others. Since the 1890s, Populist historiography has followed the same pattern. Domesticators (most importantly John R. Hicks' *The Populist Revolt* [1931]) make Populism an accepted part of American political life, whose economic demands were slightly in advance of their time. Discrediters (most importantly Richard Hofstadter's *Age of Reform* [1955]) make Populism a symptom of mass political irrationality. The two responses have a common core. Both share commitments to orthodox political development and to modern democratic theory, commitments which Populism calls into question. In terms of development, both ignore the singular importance of the historical moment in which Populism appeared; they deny, that is, that Populism proposed a workable alternative course for political and economic development, and that its failure fixed "the System of 1896" on America—a system of low mass political involvement, and of two parties committed to the economic power of capital. As democratic theory, both deny the healthy, transforming possibilities of a third-party mass movement against established political elites.

Lawrence Goodwyn's splendid and definitive history restores to Populism its authentic character. Goodwyn rescues Populism from the "shadow movement" into which domesticators and discrediters have collapsed it. The Farmers' Alliance, organizing and defending marketing cooperatives, mobilized hundreds of thousands of southern and western dirt farmers. Over 40,000 sub-alliances—one in virtually every southern county—brought farmers into the Alliance lecturing system and made them readers of the reform press. Mass political education and activity created a "movement culture." In their "unsteepled places of worship" (p. 605)—the phrase is E. P. Thompson's—farmers created a culture alternative to the dominant one. Ordinary folk, heretofore (and subsequently) excluded from economic and political power, gained practical hope and self-confidence from working together. Discovering through the failure of their co-ops the power of finance capital, Alliance farmers moved to radical politics. The major parties, pervaded by political nostalgia, and with slogans like "the bloody shirt" and "the hardy yeoman," mystified the actual conditions of American life. Thanks to the strength of movement culture, Populism broke with "the party of the fathers" and saw the dirt farmers for the "laborers" (p. 120) they were.

Lacking movement culture, the shadow movement never strayed far from conventional wisdom. It responded to Populist power from within the dominant political culture. The people and programs most often used to domesticate or discredit Populism were not Populists at all. Among the shadow movement's adherents were: such major party politicians as the racist Ben Tillman and the fundamentalist, silver Democrat William Jennings Bryan (racism and free silver were rooted in the dominant culture; Populism was more resistant than the shadow movement to both); the silver lobby, financed by silver mining interests, and employing the silver pamphleteer, W. H. (Coin) Harvey (whose crank theories Hofstadter used to tar Populism); and a few Populist officeholders who owed their election to non-Populist votes and were less committed to the third party than to their own political survival.

Democratic Promise undermines three major props of modern democratic/development theory. First, it establishes that a radical, mass uprising of ordinary farmers had more democratic positions on issues like race and electoral rights than the established elites. Second, it puts in proper perspective the distinction between programmatic party on one hand, amorphous party/narrow interest group alliance on the other. Individual politicians (as opposed to what Maurice Duverger called party militants) lacked a movement base; they easily capitulated to silver money and electoral ambition, at the expense of the third party and its coherent program. This was disastrous—and its significance was buried in subsequent theory and politics—because at stake was the democratic political control of capital. Populism attacked the (by no means inevitable) move to industrial combination, and favored government ownership of the means of transportation and communication. The farmer was caught in a web of credit—restricted currency, farm mortgages, and the southern crop lien system (which made the farmer a literal serf of the merchant). Populism attacked finance capital, and proposed a flexible, small-producer-oriented, state-controlled currency and credit system—not to be confused with the free-silver panacea.

Goodwyn is a voluntarist. He explains Populist success and failure by organizational penetration rather than "economic determinism." Yet some types of farmers were more receptive to Populism than others; voting data could illuminate the relative weight of organization and demography. Goodwyn also underplays Populism's apocalyptic tone, omitting, for example, the purplest passage from the preamble to the St. Louis platform. He recognizes, nonetheless, the fundamental demographic difficulty that contributed to apocalyptic rhetoric—Populism's weakness among industrial workers. Populism, Goodwyn shows in a striking piece of original research, was born from the great southwestern railway strike of 1886. The strike radicalized the original Texas Alliance-generated Populist talk of "revolution" (p. 63), helped dirt farmers conceive of themselves as workers, and began a continuing alliance between Populism and radical labor. Ironically, however, 1886 was—around the country—the failed revolutionary moment for the working class. Populism rose as radical labor fell, and without more working-class support, Populism had to fall too. Domesticators and discrediters, retrospectively vanquished in thought, were victorious in life.

MICHAEL PAUL ROGIN

University of California, Berkeley

Political Science and School Politics: The Princes and Pundits. Edited by Samuel K. Gove and Frederick M. Wirt. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. x + 143. $14.50.)

Gove and Wirt bring together four research-oriented papers by competent political scientists and educators with insightful comments by practitioners of equal scholastic ability. Their primary focus is on processes of decision making producing educational change by local, state and federal policy makers. A fifth paper applies John Rawls' theories of justice to a pertinent philosophy of education.

In "Horse before Carts: Developing Adaptive Schools and the Limits of Innovation," Willis Hawley concludes that imposed system-wide innovation results in inadequate teacher implementation and insignificant learning changes. Teacher adaptation is more important because it reduces peer conflict, minimizes organizational constraints and realizes greater satisfaction. P. T. Hill, commenting on Hawley, emphasizes the pressures on schools for innovation to receive federal and state aid and thinks few schools can be adaptive unless teachers are given greater incentives with the cooperation of parents and students.

"Professionalism, Community Structure, and Decision-making School Superintendents and Interest Groups" by M. O. Boss, H. Ziegler, H. Tucker and L. A. Wilson charges social scientists with negligence in research concerning professional experts as major sources of power and in the role of superintendents in resolving conflicts produced by community interest groups. The paper implies that high-status communities give greater support to educational values and that superintendents with doctorates experience greater management conflict. F. G. Burke disagrees with this characterization, holding that educational values have little relation to community structure and that the most significant change in local decision making is "the rapidly evolving nature of our political culture. We are entering an era of participatory consensus" (p. 66).

"Political Perspectives on Recent Efforts to Reform School Finance" by Donna E. Shalala and Mary F. Williams points up the increasing influence of state governments on local school boards since legislatures "have emerged as the dominant policymakers in educational finance" (p. 75). There is little evidence of future centralization by the federal government through financial control of schools. J. S. Berke praises Shalala and Williams but adds that the courts forced attention on change for greater equality in education and that Title I ESEA provided substantial federal aid to needy urban areas and stimulated new state aid formulas. "Probably in no other major field of domestic policy has the impact of policy research been as influential in the process of change as in school finance" (p. 86). Furthermore, research is increasingly interdisciplinary: "policy analysts have adopted computer simulation technology to providing information on the likely effects of a variety of potential formulas, thus permitting policymakers to draft legislation with considerably greater knowledge of its tax distributional effects than has ever been available before" (p. 88). The "dozen or so scholars" (in school finance reform) exhibit "a sensitivity to political reality, a firm grip on quantitative methodological skills, and an ability to move comfortably in both the worlds of research and policy. . ." (p. 89).

In "Don't Trouble Me with the Facts': Congress, Information, and Policy Making for Postsecondary Education," Thomas R. Wolanin concludes that by its very nature Congress does not and the Office of Education cannot supply needed information. Educators and legislators live in two different worlds "separated by different time horizons and styles that inhibit the flow of information. . ." (p. 107). Congress members do not ask the right questions; "social scientists have not done very well in predicting human behavior" (p. 95). Therefore, "decision makers operate in the realm of informed guesswork" (p. 95). Congress must deal with too many issues in too short a time under an adversary system and without adequate staff assistance. Samuel Halperin points out that Congress has produced massive programs in education but its members tend to legislate what they already believe, what appeals to them, serves their value preferences and coincides with what they think is needed.

Tyll van Geel in "John Rawls and Educational Policy" first summarizes what he thinks is dominant in the social philosophy of education as a background for discussing Rawls' thought in *A Theory of Justice* (reviewed in "A Spectrum of Responses" in the *American Political Science Review* (1975): 588–674). Van Geel finds that Rawls includes equality of educational opportunity in his concept of equality and liberty and that government must give children additional aid to overcome their socioeconomic backgrounds. He concludes that "reason should be used in the process of formal schooling, and the end result should be children who not only accept the principles of justice but also have a capacity for moral reasoning" (p. 141).

In light of the four articles based on research and experience, one wonders to what extent superintendents and other administrators, members of school boards, state legislatures and Congress actually give much thought to Rawls' concepts.

J. OLIVER HALL

Michigan State University

The Indiana Voter: The Historical Dynamics of Party Allegiance During the 1870s. By Melvin Hammarberg. (Chicago and London: University of Chicago Press, 1977. Pp. 251. $17.50.)

Those active in the movement of recent years to promote the application of social science concepts and methods to the study of the past will find much to admire in Melvyn Hammarberg's study of the Indiana voter in the 1870s. His book draws imaginatively from social science literature on contemporary voting behavior, especially the work of the authors of *The American Voter,* and it applies statistical analysis with skill and understanding rarely displayed in the work of historians. *The Indiana Voter* is based primarily upon an analysis of two sets of historical statistics. One includes county-level election, economic, and demographic data which provide an overall view of political behavior and relationships in Indiana in the 1870s. The other data set is a sample compiled by the author of individuals drawn randomly from the 1870 manuscript census returns of nine Indiana counties. Data from the manuscript census were combined with additional information provided by a set of volumes published in 1874 called *People's Guides* that were available for the same nine counties. Subsequently, the data were weighted to produce a sample that would accurately represent social and economic groups in the total population. The author emphasizes that this second body of data is "a biased one since it is based upon nine counties that only partially represented the whole state." It is, nonetheless, a uniquely rich set of data that provide much new information and understanding about the mid-nineteenth century voter, and analyzed in the state-wide perspective provided by aggregate county-level data, Hammarberg's findings permit an analysis of the Indiana voter that probably at least approaches the representativeness of contemporary survey data.

Hammarberg examines the validity of two major schools of interpretation of voter behavior in mid- and late nineteenth-century American politics—one being the economic interpretation, the other approach emphasizing ethnic and cultural differences as major determinants in voting. He concludes that while both interpretations can be partially supported by his data, neither interpretation sufficiently accounts for the complexity he finds. Wealthier farmers were more likely to be Republicans and poorer farmers were more likely to be Democrats, for example, but other differences among voters, especially occupation and place of residence, seem "much more important." Ethnic and religious factors, as well, seem to "explain" the behavior of some voters; but, on the other hand, almost half of Hammarberg's sample did not identify themselves with any denominational subgroup. Membership in a denominational subgroup, in fact, seemed more significant than the kind of religious group voters belonged to, and institutional associations such as religious denominational activity were of "crucial import" in the dissemination of partisan attitudes. Hammarberg concludes that farm voters, unlike those in the small towns and villages, associated less with others and hence received less political information, were less likely to have a strong attachment to either of the two major parties, and were more likely to respond to the appeal of short-term, third-party movements than other groups in the Indiana electorate.

To be sure, these findings apply only to one decade in a single state and thus suffer from the limitations inherent in all case studies. But *The Indiana Voter* is an excellent work of its kind and should be examined carefully by students of the history of American voting behavior.

HOWARD W. ALLEN

Southern Illinois University

Party Leadership in the States. By Robert J. Huckshorn. (Amherst: University of Massachusetts Press, 1976. Pp. xvi + 301. $15.00.)

The office of state party chairperson has, during the past decade, emerged from relative obscurity. From a situation in which many state parties had no fixed headquarters and whose chairs were usually unpaid part-timers with little or no staff, today's state party chairperson is more readily recognized as a person of political substance. Although some observers perceive contemporary parties as being in a state of "decomposition," the state party organizations, so often called the "building-blocks" of American parties, have been enjoying a resurgence of activity. Much of this has been due to the increasing competitiveness of state politics which has led both major parties to throw more resources into their permanent structures. The "reform" of both national party organizations has, furthermore, led to the expansion of the national committees to include state chairs. Thus state chairpersons gained something of a national forum and have even developed their own national associations.

In this book Robert Huckshorn presents the most thorough study to date of the state chairperson and from the chair's perspective, of the regular state party organizations. Huckshorn brings to the study his experience as a fellow of the National Center for Education in Politics with the Republican National Committee as well as an assistant director of the NCEP. Those positions gave him the entree which enabled him to interview the chairpersons of some 80 state parties, divided evenly between the two major parties. It is on these interviews, conducted between 1969 and 1972, that the study is primarily based.

We can find in the study answers to all of the basic questions about the chairs themselves: their education, occupation, sex, mobility, and career aspirations. On most counts they appear much like other middle-range American politicians, those in Congress or in state offices. While there are some notable exceptions such as John Bailey in Connecticut, their tenure is typically brief, averaging about three years. They leave office for a variety of reasons, but it is clear that American party organizations cannot live a life too independent of the flow of candidacies for elective office. Indeed a substantial proportion of state chairpersons run for office themselves, often successfully. The overwhelming concentration of such electorally ambitious party officials in competitive as against one-party dominant states is good evidence of the extent to which competition strengthens organizations by attracting and arousing seriously ambitious politicians.

Huckshorn discerns three roles for party chairs: the "political agent" who serves the incumbent governor; the "out-party independent" whose party does not control the governorship; and the "in-party independent" who is able to develop an organization with a life of its own free of the particular governor in power. It is, of course, the latter type which is most interesting because American politics has not been receptive to such independent organizational strength. Huckshorn asserts that it exists in some 30 percent of his cases. It is not clear, however, precisely what he means by this, for he asserts (p. 85) that in four states *both* chairs were in-party Independents, an impossibility by definition since no state has two governors. Nor does he tell us, in accordance with his agreement of confidentiality, which states are in which categories. Thus we are left with discussions of the more publicized examples, which include such disparate parties as the John Bailey-dominated Connecticut Democratic Party which has fostered many a career from Bowles to Grasso, and the regular Democratic Party in Alabama, chaired by Robert Vance, in intense opposition to Governor George Wallace. Nor does Huckshorn do much more with the distinction than to outline them as "role-orientations." His subsequent analysis of the sorts of things chairpersons do in campaigns, recruiting candidates and the like, rests more on the distinction between Democrats and Republicans. As one might expect, given the parties' basically similar organizations, the distinctions are not great.

This is, therefore, a good study of the group known as state party chairpersons circa 1970. We learn who they were, what they do, what they think is important about their jobs. The author quotes liberally from the transcripts of his interviews. We also have the best account of their emergence on the national scene as a bloc within the national committees and their efforts to develop lateral national and regional organizations. It is, however, strictly the party chairs' point of view which dominates the analysis, and therein lies the principal weakness of the book. It is written from the perspective of only one actor in state parties, and not necessarily the most important one. Party chairs, quite naturally, like to think in terms of filling out tables of organization, of meetings held, of monies raised, and of issues publicized. When a national party official asked the author to rate the state chairmen of one party he responded to one of Huckshorn's high ratings with the statement, "Has he won anything? He can't be effective unless he wins elections!" (p. 264) That Huckshorn was "startled" by the response is a clue to his own perspective.

The book deals with the party chairpersons as an aggregate. As a result, there is no way in which other political scientists at work on state politics can build on or use the study for the comparative analysis of party organizations. Undoubtedly the work gains from the openness of the interviews given in the confidentiality of aggregate reporting. But it also loses much because, while we can find out here such things as what proportion of chairpersons make endorsements of candidates in primaries, how many raise funds, how many deal with patronage matters, and so on—we learn nothing about which state party chairs do those things. We will begin to develop the study of comparative state parties only when we can begin to relate such variations to the vast number of other variables extant.

JOSEPH A. SCHLESINGER

Michigan State University

Red Tape: Its Origins, Uses, and Abuses. By Herbert Kaufman. (Washington, D.C.: The Brookings Institution, 1977. Pp. 100. $7.95, cloth; $2.95, paper.)

In this little volume of three chapters and exactly 100 pages Herbert Kaufman deals with a pervasive and rapidly growing albatross of our society, of our government, and particularly of our federal government. Red tape is a phenomenon almost universally deplored and loathed, but, as Kaufman points out, there has been rather little explanation of its sources and its causes. Without a better understanding of where it comes from and why, the common responses are simply exasperation, castigation of the public officials thought to promote it, and feelings of helplessness.

Kaufman's basic theme is that "we have met the enemy and it is us." It is not the bureaucrats, many of whom deplore red tape as much as the rest of us, nor members of Congress and politicians. It is rather a response to the demands of the myriad groups and interests in the society which force its production to promote and protect the governmental purposes they support. Much if not most red tape is generated to assure values which most of us heartily endorse. Among the sources suggested by Kaufman are: protecting the public (which includes preventing injuries, maintaining the purity of food and drugs, assuring safety in transportation, etc.); overseeing fair dealings between buyers and sellers, unions and their members, banks and depositors, universities and students; alleviating distress (that of Indians, the unemployed, children, the aged, the blind, the disabled, and victims of disaster); maintaining representativeness (participative democracy) and due process of law; keeping government public; upholding honesty and protection against theft and fraud; and striving for equity in taxation.

Kaufman finds no easy panacea and is distinctly skeptical of four sweeping proposals that have been suggested for the reduction of red tape. They include broad-scale reduction of governmental responsibilities, which he thinks unlikely and probably unrealistic. Others are devolution of federal responsibilities to state and local units as in general revenue sharing, concentration of authority in "czars" or other authorities with power to "cut through" red tape, and manipulation of pecuniary incentives (primarily through the tax system and subsidies) so that private interests would serve public purposes. Kaufman doubts that any of these would, in the long run, significantly reduce red tape. He pins his faith on political and administrative processes to continually peck away at the beast and prevent its growing beyond all control, a frank acknowledgement that it cannot be eliminated.

It is interesting that this book was published at about the same time as the final and summary report of the Commission on Federal Paperwork (October 3, 1977). Though paperwork and red tape are not synonymous, paperwork is a major component of red tape and an evidence of much of it that is not strictly defined as paperwork. The commission, after exhaustive two-year study, offered some 770 specific recommendations which it believed could reduce by 10 percent the total cost of federal paperwork in the government and the private sector—a total it estimated at more than $100 billion annually. The commission directed its major thrusts at faulty organization, poor management, and unnecessary procedural obstacles. Its reports were far less generous than Kaufman's toward the reasons and justifications of paperwork and far more caustic and replete with horrible examples.

The two reports are not necessarily inconsistent, though they view the problem from differing perspectives and have different purposes. They should be read in tandem. Kaufman's work—another example of the new genre of style he is bringing to the Brookings Institution—is written with grace, lucidity, and brevity. It is easy and profitable reading.

FREDERICK C. MOSHER

University of Virginia

The Effects of Mass Communication on Political Behavior. By Sidney Kraus and Dennis Davis. (University Park: Pennsylvania State University Press, 1976. Pp. xiii + 308. $16.50, cloth; $7.95, paper.)

Political communication is not a new area for political scientists, but it is one which never seemed to enter into the mainstream. Originally, the concern was with propaganda and the ideological aspects of the media; that proved to be time-bound. Today the concern is with socialization, voting, participation, social change, and public opinion. Here, research seems to be well on the way toward establishing an important, if not central, role for the mass media. While political scientists have made, and are continuing to make, contributions in this field, much of the important work continues to be done by researchers in other disciplines. The merit and usefulness of this book is that it brings to political scientists in a usable form the considerable relevant work being done outside our discipline.

Kraus and Davis cite over 700 books, papers, articles, and reports in a critical survey extending through the book's eight chapters. In addition, well-annotated bibliographies are appended to five of the chapters. The authors, both in the Department of Communication at Cleveland State University, are eclectic in the sources they bring together, although there are boundaries as to the subjects covered. The book's thesis is that mass-media variables (and especially those relating to television) are major factors determining American political behavior. Research is reviewed from that point of view. The related areas of media content, news processes and the news business, and the foreign affairs aspects of the mass media are consequently neglected. Further, the authors make little attempt to differentiate among research findings on the effects of the several mass media. The several communications models are discussed in terms of theory, but little attention is paid to psychological explanations of media effects. Given the authors' concerns, these omissions are excusable. The book argues convincingly that mass media are important to an understanding of recent American political behavior. There seems to be little question that future voting, socialization, and participation studies will be giving greater emphasis to media variables.

The weakest aspect of the book is its subsidiary thesis that the major voting and political socialization studies done in the 1940s, 1950s, and 1960s were systematically flawed, which means that the effects of the mass media were played down. Why media variables have only recently come to be seen as important in these areas is something which should be explored. However, suggestions that "academic researchers tend to investigate only variables which are considered to be within the 'acceptable domain' of their separate disciplines" (p. 8), that research is often "self-serving" (p. 249), and that social science has been "corrupted" (p. 247), add little light to the subject. There are better reviews of the voting behavior and political socialization research and one would hope that individuals unfamiliar with the work in those areas will find their way to them.

While the research on the effects of mass communication on political behavior is rapidly being augmented by newer work, including a number of media-centered studies of the 1976 elections, the literature covered in this book will remain of interest to future researchers for some time. Scholars directly concerned with the mass media and those more generally concerned with American electoral behavior will find the book particularly useful. It is well indexed.

R. DARCY

Oklahoma State University

Black Ballots: Voting Rights in the South, 1944–1969. By Steven F. Lawson. (New York: Columbia University Press, 1976. Pp. xii + 474. $20.00, cloth; $6.95, paper.)

Despite its stated intention to emphasize the role of civil rights organizations, *Black Ballots* is a general survey of voting rights as a national political issue. Steven Lawson may have in fact too willingly followed voting rights across the national political landscape. Although the book represents a prodigious effort (documented in 75 pages of footnotes of largely unpublished materials), we end up covering familiar terrain. Major sections are given to descriptions of litigation dismantling the white primary, the poll-tax debate in Congress in the late 1940s, passage of the 1957 and 1960 Voting Rights Acts, Justice Department enforcement of voting rights in the early 1960s, and the credential dispute at the 1964 Democratic convention. Not much new is revealed here, and the prominent journalistic accounts of these events such as those by Evans and Novak, Berman, and Navasky remain more convincing. It is unfortunate that the initial emphasis on civil rights organizations is abandoned early on amid the intricacies of congressional and administrative politics. We could use a good study of strategy making within and among the major organizations during the various phases of the civil rights movement.

Lawson's interpretation of the major events is hampered by a rather naive conception of American politics. The circuitous path of voting rights over the decades is viewed as reflecting the opportunism of politicians rather than the incremental character of policy making, especially when there is substantial opposition. The following examples illustrate the author's overemphasis on political expediency. In late 1955 Attorney General Brownell proposed formation of a civil rights commission and authority to seek injunction relief for victims of voting discrimination. We are told that his principal reason was to court black support for Republicans when it appeared that President Eisenhower's heart attack had taken him out of a reelection attempt. Although Brownwell and others within the administration were clearly worried about their party's chances without Eisenhower, there is no direct evidence offered that this was the inspiration for Brownwell's proposal. Lawson simply argues that Brownwell, described as a "mastermind" political strategist, must have been politically motivated. Another problem with this interpretation is that Brownwell persisted after Eisenhower's recuperation. In fact, he persisted in congressional testimony even over Eisenhower's opposition! With a similar emphasis, the author observes that Senator John Kennedy "enhanced his image" among the southern Senate Democrats by favoring an amendment to the 1957 bill that required a jury trial for contempt cases. Kennedy's explanation that such a compromise was necessary to prevent a southern filibuster is relegated to a footnote in the back of the book. As a final example, Senator Frank Church and fellow western Democrats are accused of selling out a strong voting rights bill in 1957 in return for southern support for a public works project, even though their votes were wholly consistent with their past and future voting record.

With politicians viewed as governed by expediency, normal governmental processes appear tainted. In describing passage of the 1957 and 1960 Civil Rights Acts Lawson places too much significance on Johnson's desire to enhance his national image while mollifying the southern Democrats, and too little on the real differences of opinion, even within regional and party groupings, which made compromise necessary. These laws are judged unfavorably against the maximum civil rights proposals, where they should perhaps be judged against the more likely alternative of no legislation at all. Similarly, the "muted" response of the Kennedy administration to the needs of civil rights demonstrators is presented more as a failure of will than a result of statutory limitations or, as Navasky has described so well, the Justice Department's inability to harness the FBI. Finally, public opinion is poorly represented by editorial blurbs from local newspapers rather than by readily available survey data.

Black Ballots suffices as a chronological guide to the main events, but it fails as interpretive history.

SAMUEL KERNELL

University of California, San Diego

Wealth and Want. By Stanley Lebergott. (Princeton, N.J.: Princeton University Press, 1975. Pp. vii + 217. $6.95.)

This book purports to be a series of essays on wealth and want in the United States. It is, however, an *apologia* for the American capitalist system. The book is characterized by a style that is supercilious, disingenuous, and is a reprehensible attempt to be amusing at the expense of the poor, the aged and the young.

The author attempts to demonstrate that "real" poverty doesn't exist and that relative poverty is a result of the free choice of the poor themselves. Furthermore, he tries to justify the skewed distribution of income and wealth on the spurious grounds that, as it has persisted for so long, it must therefore represent the democratic choice of the citizenry.

Illustrative of Lebergott's approach is his assertion that a major cause of poverty in the United States has been the Social Security system. He arrived at this remarkable conclusion by claiming that the Social Security system has induced people to retire rather than work and has persuaded the aged to live apart from their children (pp. 15–16). Another cause of poverty cited by the author is the failure of families to put their children out to work or to take in lodgers (p. 13).

As evidence of the fact that the distribution of income in the United States is democratic, Lebergott argues that the pattern of welfare care has been consistent over decades. He has the gall to term this meager welfare support "guaranteed income." He supports his contention as follows:

> We may conclude that a guaranteed income has long prevailed in the United States. . . . The level of that income appears to have run from 25 to 30 percent of a common laborer's earnings, and it has done so for more than a

> century. In a democratic society that persistence suggests the ratio is consistent with the actual preferences of a majority of Americans. . . (p. 60).

Even democratic pluralists do not go to that extent to avoid a recognition of the long-run/ systemic causes of poverty.

In another section, after noting the meager assistance provided to the blind, the author comes to accept with equanimity that our society will also fail to do justice to the poor. In fact, he implies we should not aid the poor, for "in contrast to welfare aid or aid to dependent children or unemployment insurance there is little likelihood of anyone choosing [blindness] in order to get free assistance" (p. 158).

One final illustration of his cavalier attitude ought to be sufficient. He seems to suggest that a little poverty, nutritionally speaking, may be a good thing. "True, the rising popularity of soul food could bring significant nutritional gains. Collard greens, for example, are ten times better as a source of ascorbic acid than frozen corn, and fantastically better than hearts of palm salad" (p. 83).

It appears that we should be grateful that the wealthy accumulate even greater wealth because, as Lebergott observes, by saving and investing rather than spending their incomes, the wealthy leave more goods on the shelf for the rest of us, and by investing, they increase the money supply and thereby lower the interest rates so we can borrow for our needs at less cost to ourselves (pp. 151–52).

That the persistence of the skewed distribution of income and the even greater skewed distribution of wealth may not be a matter of free democratic choice on the part of the poor or of average citizens, but may be a result of the malfunctioning of the economic system, seems to have escaped the author's notice. Thankfully, other authors are dully concerned about the inequities of the American political economy (e.g., Raymond Franklin, *American Capitalism*, Random House, 1977, p. 117). As was noted in *Business Week*, 1972:

> In almost every year since 1947, the poorest fifth of American families has received only about 5% of the country's total family income while the top fifth got 42%–an 8:1 ratio. . . . From 1949 to 1969 . . . the gap between average real incomes of the poorest and richest fifths of the population widened from less than $11,000 to more than $19,000 in constant 1969 dollars. . . . According to the latest available survey, the top 20% of consumer units . . . owned 77% of all wealth. . . . The richest 2.5% of U.S. families owned 44% of all private assets, while the poorest 25% have, on the average, no net worth at all–their total debts just equal their assets.

It is unfortunate that a topic as problematic as poverty in a nation with such wealth is treated so disingenuously. Suffice it to say that such differing economists as Milton Friedman and Kenneth Galbraith have taken the matter seriously. If one were to give credence to Lebergott's book, one would be forced to confront the choice between either celebrating the American experience as he has done or be prepared to advocate some drastic revolutionary changes in our economy and our politics.

Stanley Lebergott's celebration of the American experience is unsophisticated and opportunistic, in spite of his pretended erudition.

CHARLES A. MCCOY

Lehigh University

Urban Politics and the Criminal Courts. By Martin A. Levin. (Chicago: University of Chicago Press, 1977. Pp. x + 332. $19.00.)

Sentencing has long been at the periphery of "hard" political science. For one thing, variations in court sentences are usually seen as politically idiosyncratic. More important, however, most of us do not see sentencing as being very closely linked to those public policies or individual actions which have an impact upon the lives of the vast majority not involved with the criminal justice system. Now comes Martin Levin who tells us that: (1) sentencing is not politically idiosyncratic; rather–in urban areas at least–considerable differences result from varying political ambiences, and (2) sentencing policies are clearly connected to matters which affect all of us such as deterrence and crime prevention, expenditure levels for prisons, insurance and welfare, and public support for the courts as dispensers of justice.

The first point is supported by extensive research in two cities with dramatically different political milieus. One is Minneapolis with its prevailing ethic of non-partisanship and clean government. There judges usually come from corporate legal practice upon the recommendation of the Bar Association; most are of middle class and Anglo-Saxon or Nordic background. The other is Pittsburgh, a long-time "machine" town. Judges are selected from the ranks of the party faithful and carefully balanced to reflect the city's ethnic composition;

most have working-class backgrounds and Bar Association recommendations receive short shrift.

As a result, sentencing and the overall felony process differ considerably between the two cities. Minneapolis judges are legalistic and oriented toward protecting "society." Plea bargaining is rare, but nonetheless guilty pleas are normal and sentences are stiff. They are, however, internally equitable; the same crime generates the same comparatively lengthy sentence regardless of circumstances (except for recividism). By contrast, in Pittsburgh plea bargaining is common and when trials occur the defendant usually claims redeeming personal characteristics rather than innocence. Private intercession with the judges by relatives, ward captains, etc., is not unknown. Probation is frequent and sentences lenient, even for recividists. Pittsburgh judges are frank about their individualistic orientation and the importance of extralegal factors. "The boy seemed like a nice chap and the car was covered by insurance," said one (p. 126); more generally, another explained, "We don't sentence the crime, we sentence the offender" (p. 129). (In this context, I cannot help being reminded of Harold Berman's description in *Justice in the USSR* [1963] of Soviet "parental law" which focuses on the defendant's whole person rather than his offense.)

Levin's observations in these two cities add to the literature on how felony processes really work. His work appears roughly simultaneously with James Eisenstein and Herbert Jacob's *Felony Justice* (1977) which reports on similar observations from Baltimore, Chicago and Detroit. Between these books and a couple of earlier studies, we are becoming aware of just how different the felony process is from city to city. In some places the judge plays a far more important role than is commonly ascribed to the judge in the typical plea-bargaining syndrome. Indeed, there is not a standard syndrome, but a seeming abundance of them—to say nothing of the fact that plea bargaining is absent in a good many jurisdictions. Perhaps we stand on the threshold of developing some profiles of (to borrow from James Q. Wilson) "Variations in Felony Process Behavior."

The second point is supported by Levin's reanalyses of other researchers' data. Other things being equal and with a few qualifications, his tightly reasoned conclusion is that in the long run probated sentences reduce recidivism considerably more than do jail terms, but the latter are a greater deterrent to increased crime. He thus stresses that sentencing "strategies bear the primary responsibility for reducing recidivism and deterring crime" (p. 180). And he recommends an optimal strategy for judges: "grant most first offenders probation, but incarcerate enough of them so as to eliminate the expectation that it was safe to commit crimes until the time of a first conviction" and "incarcerate all second offenders for long terms" (p. 183).

Despite its value, there are several irritating characteristics to this book. For one thing, all 77 tables are placed at the book's end, which virtually requires the reader to thumb through two sets of pages simultaneously. For another, the discussion of politics and the courts in the two cities tends to repetition and redundancy. More bothersome yet is the fact that Levin conducted his research in 1966; the undue publication delay is probably no one's fault in particular, but the data are now over a decade old. Since then there has been an almost revolutionary change in the provision of (and often the nature of) defense attorneys in this country, but Levin can tell us little about how this affects urban felony processes. Presumably for the same reason, Levin often does not incorporate or discuss at length some of the more recent literature regarding urban politics, judicial recruitment and criminal justice.

The book's most striking characteristic, however, is its schizophrenic nature. The first half of it is deeply immersed in the particular political and felony processes prevalent in Minneapolis and Pittsburgh. But the discussion of the impact of sentencing upon deterrence, recidivism and expenditures is highly general—in the nature of a universalistic rational model. The efforts at connection are quite strained and the discontinuity is even more pronounced because Levin never fully discusses the implications of his sentencing recommendations for the two cities or urban America generally. It's almost like buying two books for the price (albeit a high price) of one. Both parts make worthwhile reading and go far toward bringing sentencing more within the realm of "hard" political science, but the whole is less than the sum of its parts.

BRADLEY C. CANON

University of Kentucky

American Politics in a Bureaucratic Age: Citizens, Constituents, Clients, and Victims. By Eugene Lewis. (Cambridge, Mass.: Winthrop Publishers, 1977. Pp. ix + 182. $10.00, cloth; $4.95, paper.)

In *American Politics in a Bureaucratic Age* Eugene Lewis has written an excellent and provocative account of the rise of public bureaucracies and the implications for traditional democratic theory of the bureaucratic state. The author has succeeded admirably in using a style that is suitable both for students and professional scholars of administration, and both groups will benefit from reading this book. The author integrates complex concepts of administrative theory around interesting historical and contemporary illustrations of the role of bureaucracy in government, and the unusually lucid style of the book makes it a joy to read.

The author begins his analysis of public bureaucracies with a personal statement about the impact of bureaucracy upon himself, a statement that immediately helped to humanize and personalize an otherwise almost inhuman and certainly impersonal subject. Students will particularly like this beginning, which provides a setting for the somewhat heavier reading that is to follow. Scholars as well as students will profit from the integration of political and administrative theory in relation to the role of bureaucracy. Dahl, Lipset, Lakoff, Mancur Olson, Theodore Lowi, Schattschneider, Bentley, Truman, McConnell, James Thompson, and James Wilson, are among those whose writings are discussed in the construction of a general theoretical framework for the book. And this theoretical framework is never abstract but always given relevance within the context of contemporary government.

The theoretical and conceptual framework of the book provides a setting for the discussion of the role of the bureaucracy in policy formation, and the author concentrates upon political economy, social welfare, and national defense to illustrate the importance of the evolving bureaucratic state. A number of policy studies and important analyses of the policy process are covered in these sections of the book, including Aaron Wildavsky's *The Politics of the Budgetary Process,* Robert J. Art's *The TFX Decision,* and works by Selznick, Banfield, Galbraith, David J. Rothman (*The Discovery of the Asylum*), Herbert Kaufman, Martha Derthick, and Graham T. Allison. This list is not exhaustive but only reflects part of the broad scope of the author's wide-ranging knowledge and use of historical and contemporary theory and analyses of government, bureaucracy, and public policy.

The book focuses initially upon the importance of the individual citizen's participatory role in government, a role derived both from democratic theory and from a more general belief in the dignity, worth, and rationality of the individual which lies behind it. The theme of the book is that the inevitable growth of bureaucracy has changed both governmental practices and traditional ways of thinking about government. The rise of the modern democratic state and the growth of public bureaucracy has tended to diminish the individual's participation in government. We have become an organizational society, in which "people interact with their government mainly through organizations or in organizationally defined settings" (p. 170). The author finds that the bureaucratic state reinforces the pluralism of the polity. Subsystem politics dominates the process of policy formation. But in policy subsystems professional bureaucrats are assuming more and more power to shape the final results of government action. The author implicitly rejects the traditional approaches of democratic and group theory, as well as such formal devices as the rule of law as means by which to bring the bureaucracy under control. The author suggests that as professional bureaucrats become increasingly powerful, the only checks and balances capable of controlling the administrative state will be those that exist among the experts themselves. This is the provocative conclusion to an excellent and worthwhile book.

PETER WOLL

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Equality and Urban Policy: The Distribution of Municipal Public Services. By Robert L. Lineberry. (Beverly Hills, Calif.: Sage Publications, 1977. Pp. 205. $11.95, cloth; $6.00, paper.)

The study of urban service delivery is one area where the empirical and normative concerns of political science clearly converge. It is also one area in which competent policy analyses can contribute significantly to our collective capacity for coping with public problems. Robert Lineberry's most recent book highlights both these facts. It fails, however, to seriously address some of the most important issues in this field of study.

The book focuses on the question of whether or not municipal public services are equally distributed among geographically defined groups within urban jurisdictions. Lineberry organizes his search for the answer to this question around three sets of hypotheses. The first group reflects the "conventional wisdom" that municipal services are unequally (and inequitably) distributed among urban neighborhoods, and that this inequality is a result of discrimination against class or racial groups or the "politically powerless" by those who exercise control over local policy processes. The second set of hypotheses stem from an "ecological" view of urban development which traces service inequalities to structural attributes of the neighborhoods (age, density, etc.) rather than the socioeconomic characteristics of their residents. The remaining hypotheses argue that services are distributed as a direct result of bureaucratic applications of decision rules developed for "professional" reasons.

Lineberry then subjects these hypotheses to the inevitable "tests against reality" by examining distributional patterns in the delivery of police, fire, library, water and sewer, and recreational services in San Antonio, Texas. In addition to asking the question of "who receives the benefits" of these services, Lineberry takes the important step of exploring the question of "who bears the burdens" by tracing neighborhood differences in property tax evaluation. The testing process is rigorous and provides a benchmark for those interested in the development of methods for urban policy evaluation.

Even so, one has the uneasy feeling that the data somehow fail to present a full and accurate picture of the quality of services available to residents of different neighborhoods. While Lineberry avoids the mistake of relying on expenditure measures of service quality, the indicators available to him are generally unsatisfying surrogates for the evaluative criteria that are likely to be applied by consumers of public services. The problem is not Lineberry's so much as it is that of all who seek to use official statistics to construct measures of public agencies' performance. Such statistics are commonly divorced from citizens' evaluations.

But what of the results? Lineberry concludes that both tax burdens and public services are roughly equally distributed among neighborhoods. To the extent that there are inequalities, they tend to run *counter* to those predicted in the conventional wisdom. In San Antonio, the poor, the unrepresented, and minorities are often favored by deviations from the standard of equal service delivery. Moreover, those neighborhood characteristics which are central to the arguments based on assumptions of class or race discrimination or political favoritism explain little of variance that does exist in inter-neighborhood service distributions. Ecological features of communities do a far better job of accounting for variance in services.

This last fact may reflect the relevance of ecological characteristics for the "professional interests" of the urban bureaucrats who make the daily decisions that finally determine actual service levels. Lineberry's discussion of the influence of bureaucratic decision rules on service allocations leaves the clear impression that the study of urban bureaucracies is far more important to the understanding of public service delivery than the study of political power in urban settings.

Finally, the author raises the question of the significance of the equality criteria for public services in a society marked by stark inequalities in the private sector. He clearly articulates the need to study the redistributive impact of providing equal services to unequal consumers.

This book is essential reading for those interested in urban policy analysis or in urban politics more generally. It is argued in Robert Lineberry's characteristically lively and concise style and written at a level suitable to undergraduate students and the uninitiated professional, yet a level not offensive to those specializing in the subfield. It is a useful contribution to this still new field because it does an excellent job of transcending the case study to speak to much wider interests.

So why should we be dissatisfied with the book? The most fundamental reason is that the very characteristics of San Antonio which make it an ideal site for testing hypotheses about inter-neighborhood service differences within jurisdictions make it a very poor site for learning about *metropolitan* service distributions. This is central because in most large urban areas, locational strategies for acquiring desirable combinations of services are pursued as often among as within jurisdictions. The demographic development of older cities, judicial expansion of the Equal Protection Clause, and the incentives of federal assistance programs have made it increasingly difficult to maintain arbitrarily stark inter-neighborhood service differences within central cities, and advances in transportation, communication and residential construction have made it increasingly unnecessary to do so. The metropolitan area has replaced the old city as the arena of competition for life-style advantages through spatial location and the instruments of that competition have increasingly become land-use planning and the power of municipal incorporation rather than "influence with city hall."

Since San Antonio is not a politically fractionated urban area, these trends do not manifest themselves there. The city is therefore not a promising place to search for an understanding of the political origins and consequences of metropolitan spatial form and its attendant service differentials. If the distributional implications of the political organization of urban space are to be understood, we must have evidence concerning inter-jurisdictional service differences in the context of a metropolitanized society. Questions of equity cannot be restricted by city limits.

RICHARD C. RICH

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Commission Politics: The Processing of Racial Crisis in America. By Michael Lipsky and David J. Olson. (New Brunswick, N.J.: Transaction Books, 1977. Pp. xiv + 476. $14.95.)

Lipsky and Olson argue that riot commissions in the twentieth century have functioned to purchase system stability while suppressing the opportunity for radical change during periods of violent racial crisis. The creation of a riot commission is perceived as a mechanism which "permits an executive to defer action while appearing to act and allows him to appear to be a leader and fulfill the needs of his constituents without taking substantive action" (p. 94). The composition of government-appointed riot commissions is seen to include high-status individuals who represent divergent yet dominant interests and who share a fundamental value consensus concerning the nature of appropriate change in the American system. These recruitment criteria, the authors posit, "tend to ensure that the administrations of appointing executives will be exonerated of any wrongdoing and recommendations will be advanced favorable to represented interests" (p. 298). While riot commissions are "official" bodies, they are not "authoritative" in that they can only recommend rather than enact policy changes. Thus, commissions are dependent upon the agreement of public officials if their recommendations are to be implemented; public officials will not automatically lose office if they ignore commission recommendations. While the authors acknowledge that government officials sometimes implement commission recommendations, their basic argument is that commission findings have not contributed materially to racial equality. On the contrary, by delaying the need for immediate and substantive governmental response, commissions are seen as functioning to "provide a forum for debating controversial racial issues without forcing any public official or city agency to do anything about those issues" (p. 77).

Predominantly this argument is derived from case studies of the Kerner Commission and local commissions established after the 1967 riots in Newark, Detroit, and Milwaukee. In the first chapter the authors reanalyze material commonly available elsewhere to develop their point that the riots of the 1960s were political in that they expressed grievances conceivably remedial by governmental action. The second chapter is a similar review of equally familiar material which the authors reexamine to provide an historical context for their argument. The remainder of the book is devoted primarily to the case studies.

Methodologically this book suffers from several serious defects. The principal hypothesis, that riot commissions deflect substantive action and fail to advance the cause of racial equality, is not systematically tested. For instance, several pages of statistics (pp. 141–45) are offered to show that the Kerner Commission did not generate significant governmental policy of social changes for blacks. However, this falls short of rigorous analysis in that no argument is advanced regarding the relevant time-series for comparison, why these particular statistics are examined and other dimensions excluded, and what effect could be expected given previous experience in either the civil rights movement or other social movements. In examining the effect of local riot commissions, Lipsky and Olson make no attempt to examine cities with riots which did not establish commissions in comparison to cities which both experienced riots and established commissions. Nor do they examine the effect of other explanatory variables, such as dominant public attitudes regarding black riots and rioters or elite and mass attitudes regarding the appropriateness of accelerated civil rights gains. Such variables could function either as intervening variables between the establishment of a commission and the effect of a commission, or as causal variables explaining both the implementation and effect of a commission. Finally, they make no attempt to examine alternative hypotheses; for instance, it is arguable that without the commissions we might have experienced an even more repressive reaction to the riots.

Regarding the information presented in the case studies, the authors offer merely three pages (pp. 23–25) explaining how this data was collected, over what period of time, and from what sources. It is unclear which elites were interviewed, by whom, how and why; whether multiple points of observation were systematically established; to what extent the authors interviewed various public samples; how the authors justified any reliance upon newspaper accounts; and what criteria the researchers used in making judgments regarding the weight to be given various pieces of information which came to their attention. Observational studies are problematic at best; at worst, they merely serve to reflect the researcher's preexisting opinions. Not dealing with issues such as these places the work more in the genre of narrative history. As such, much of the detail provided (especially voluminous in the local case studies) is of some historical interest. However, the case studies do not fall within the rigorous methodology of social scientific observation.

Underlying the book's argument is a radical criticism of pluralist theory which is mentioned early and partially developed in the concluding chapter. The authors seem to believe that "opportunity for decisive leadership and qualitatively different decisions about national priorities come only in crisis situations" (p. 148); as such, they regret "the loss of opportunity that may not soon rise again" (p. 148). The extension of this principle would value those governments which respond to violent domestic threat by implementing policy changes which acquiesce to the interests and/or grievances motivating that violent expression. Reflection upon this extrapolation makes it clear that while the authors generalize from their case studies, they are still engaging only in post hoc theory, attempting to explain these particular riots and commissions. One wonders what position they would take regarding a commission established to investigate violence from the right. Would their riot/commission model demand them to argue that such commissions would again function to delay decisive action, or would their pluralist model expect a different result if the majority were in favor of the interests underlying this type of rightist violence? If the delay function were posited for instances of violence from the right, would the authors deplore it as extensively as they do in the case of the 1960s race riots? By not exploring questions of this nature, the authors give us neither a general theory of riots and riot commissions nor a stimulating examination of pluralism. Rather we merely see pluralism berated once again because someone wanted to see a change which did not occur precisely as desired.

Essentially the same theoretical argument was published by these authors in a much more concise form eight years ago (Lipsky and Olson, "Riot Commission Politics," *Trans-action*, July/August 1969, pp. 8–21).

JEROME R. CORSI

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Public Interest Lobbies: Decision Making on Energy. By Andrew S. McFarland. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1976. Pp. 141. $3.00, paper.)

The question of the "public interest," particularly in relation to energy issues, has been subjected to intense political debate. By the early 1970s, citizens' groups were challenging existing energy decisions, questioning national priorities, and publicizing energy-related matters. In this book Andrew McFarland focuses on seven mass-membership public interest organizations and attempts to analyze their energy-related goals and activities. These groups include: Common Cause, Ralph Nader organizations, The League of Women Voters, The Sierra Club, Consumers Union, Consumer Federation of America: Energy Policy Task Force, and Americans for Energy Independence.

According to the author, public interest groups have acquired substantial power since 1965 and he applauds this development as a significant means of providing representation for diffuse interests in a pluralist democracy. In the first chapter, McFarland outlines the major factors which he believes have contributed to the growth and influence of citizen activities: an enlarged, college-educated citizenry, a corresponding increase in issue-motivated political actions, a decline in popular trust of governmental institutions, skillful leadership, advances in communications technology, and economic prosperity. However, there is reason to believe that these are inadequate explanatory factors. For example, McFarland ignores the impact of the civil rights and antiwar movements as well as the effect of liberalized judicial doctrines regarding qualifications for bringing suits to court. In addition, the author does not substantiate his conclusion that public interest groups have, in fact, gained power within the decision-making process. He seems to imply that membership size and enlarged budgets can be equated with power. The crucial question, one which the author fails to address, would seem to be the following: what has been the actual impact of public interest organizations on energy decisions?

Equally disturbing is McFarland's discussion of representation and the "public interest" in chapter 2. While he argues that public interest groups serve to balance the power of narrow, economic concerns in the political process, he never clearly explores the complex nature of the "public interest." Rather, McFarland's position is that several different and conflicting public interests exist on policy issues and that, at times, public interest groups represent special interests (i.e., environmental organizations lobbying against a project which cost-benefit analysis shows to be beneficial to the genral public). Alternatively, he claims that groups such as the oil industry may represent the public interest. The confusion is compounded by his inclusion of the Energy Task Force of the Consumer Federation of America and Americans for Energy Independence as "public interest" organizations. The former appears to be a coalition of special interest lobbies and the latter is closely tied to energy industries, particularly nuclear power and electric utilities. Moreover, he fails to consider some of the problems associated with cost-benefit analysis such as the tendency of government agencies to emphasize economic rather than environmental impacts of proposed projects.

The central thesis of the book, explored in chapters 3–6, seems to be that organizational characteristics (i.e., structure, resources, and membership composition), adherence to what he calls "civic balance" and a tendency toward consensual decision making, determine the stands of public interest groups on complex energy decisions. A "civic-balance" system of beliefs, according to the author, is based on the assumption that particular issue-areas are controlled by special interest elites to the detriment of common interests; thus the role of public interest groups is to counteract the power of the former. Consensual decision making, also typical of these organizations, further limits the positions that can be taken.

McFarland analyzes the energy policies of the seven groups in light of these constraints. He finds, for example, that despite conclusions by Common Cause that there should be a gradual deregulation of natural gas, civic-balance beliefs precluded active support for such a policy. Similarly, because members of Common Cause preferred to use their limited resources for the organization's first priority concern–government accountability–local chapters were discouraged from supporting state-wide nuclear moratorium initiatives. Moreover, endorsement of a nuclear moratorium would have been internally divisive and Common Cause develops its lobbying positions by preponderant majorities. McFarland also believes that the Nader organization's focus on nuclear power as well as the limited energy activities of Consumers Union are related to organizational imperatives.

Although organizational characteristics may constrain citizens groups, the usefulness of McFarland's concepts is extremely limited. He does not solve very much by showing that Consumers Union, whose major concern is consumer affairs, has few resources for issues related to energy policy. Nor does the concept of civic balance add significantly to an understanding of public interest groups. It would seem self-evident that public interest organizations perceive their function to be that of lobbying against narrow economic concerns. And while consensual decision making may limit certain energy activities, McFarland does not provide adequate information about the groups' membership preferences. For example, do members of Consumers Union even have an interest in pressure politics or do they join primarily to receive advice on products from its journal *Consumer Reports?*

Furthermore, McFarland relies extensively on information from mass mailings (generally used for fund-raising purposes) and other official documents for his analysis and conclusions. While these sources are somewhat informative, they offer incomplete pictures of the decision-making processes and goals of public interest organizations. One would have only a partial–some would say distorted–understanding of the oil industry by analyzing bulletins published by the American Petroleum Institute.

In spite of its shortcomings, the book does offer some useful insights on public interest groups in a brief and readable form. McFarland is now working on a book about Common Cause–one hopes that his next study will treat the issues more comprehensively.

LAURA KATZ OLSON

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Presidential Power and Politics. By William F. Mullen. (New York: St. Martin's Press, 1976. Pp. viii + 294. $12.95, cloth; $4.95, paper.)

This book does not break new ground. There is little new evidence or information, nor are there new theoretical formulations. What the volume does present is a fine summary of information and argumentation concerning the modern presidency. The assessment of the arguments is fair-minded, as is the appraisal of the extraordinary events of recent history. The book is an excellent text for an undergraduate course on the presidency; however, its limited treatment of the literature makes it less satisfactory for the graduate level.

The author begins with a brief review of constitutional provisions and presidential history. He notes that until very recently response to presidential aggrandizement has been almost uniformly positive. When presidents have been ranked, those appeared as "great presidents" who had been strong leaders and who had expanded the powers of the office. Journalists and scholars have been nearly unanimous in justifying the growth of presidential power and in finding reasons why there should be further expansion.

In the second chapter, Mullen presents a detailed analysis of the sources of presidential power, including the development of legislative proposals, the dominance over the budgetary process, the impoundment of funds, the "reprogramming" of funds, the veto power, patronage, the control of information, the various emergency powers, the (usurped) war-making power, and the invocation of executive privilege.

The next chapter examines the relationship between the president and the people. "The president is powerful in part," writes Mullen, "because the people as a whole expect him to be; in fact in most cases they now demand it" (p. 110). Political socialization "is the key to public expectations about what presidents should be like, how much power they have (and should exercise), what they should be able to accomplish, and their proper relationship to others (including citizens) in the political system" (p. 111). Mullen reviews the political socialization literature from the Easton-Hess surveys to the most recent Gallup findings. While the popularity of particular presidents is subject to considerable fluctuation, the popular perspective on the "generalized" president is still one of early and lastingly learned deference and affection. Indeed, as the last months of the Nixon administration have demonstrated, there appears to be a block of presidential partisans—perhaps as much as one-fourth of all citizens—who will support a president irrespective of objective events and conditions, solely on the grounds of his holding the office. The agonized pleading "but he is the PRESIDENT!" is well remembered from the last Nixon days.

Political socialization does not stop with childhood, but school and home are replaced by the news media or primary sources of political information. The media, however, focus just as much on the president as did the former sources. The singularity and visibility of the president easily outdistances the multiplicity of leaders in Congress and the Supreme Court in attracting media coverage. "The vast majority of news about the national (and international) political system that reaches the eyes and ears of the average citizen tends to be about the president" (p. 127). The relationship between the president and the press is symbiotic. The president provides stories for the press. The press provides access to the people. Presidents quickly learn to take advantage of the media and to manipulate these channels of access and persuasion.

Childhood and adult socialization combine to create in the public mind an image of the president of singular and overpowering dimensions. Mullen examines the question whether Vietnam and Watergate have weakened the mythical image of the president. He declines to give a definite answer, but has doubts that a significant weakening occurred. This reviewer, for one, shares these doubts.

Chapter 4 deals with the isolation and imperial demeanor of recent presidents. There are numerous possible explanations for the blunders of Lyndon Johnson and Richard Nixon, many of them relating to matters of personality and the presidential selection process. There is an element of truth in all of them. But the account is incomplete without considering the effect that the modern White House has on the incumbents—or, at least, on some of them. The author quotes former White House staffer George Reedy, that it is an inescapable inference "that the White House is an institution which dulls the sensitivity of political men and ultimately reduces them to bungling amateurs in their basic craft—the art of politics" (p. 158).

Mullen discusses the various causes of this dulling effect: the opulence and royal trappings of the office (Mullen compares the modern president to the Sun King), isolation and loss of touch with political reality (presidents easily come to believe in their own special wisdom, the existence of which the PR people loudly proclaim), the decline of the cabinet and the growth of the White House staff (the staff being more protective and subservient than the cabinet).

The last chapter is devoted to the question of reforms. There are useful discussions of the likely effects of changes in the presidential term of office, the adoption of a plural executive system, the inclusion of congressional leaders in the cabinet, the revitalization of the cabinet and de-emphasis on the staff system, more independence for the Department of Justice (to better prevent "White House horrors"), the strengthening of Congress, and the demythologizing of the presidency. All these reforms, of course, aim at a less powerful president. They reflect the widespread desertion of the theory of the strong presidency, dominant for so long in academic and journalistic circles. Mullen rightly thinks that we cannot solely rely on electing better people, but that structural reforms are required. Successful reform, however, Mullen also points out, will require changes along an additional dimension: greater popular vigilance and participation in the political process, a strengthening of the party system, and a better understanding that the president is *not* the whole of politics.

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Revenue Sharing: The Second Round. By Richard P. Nathan, Charles F. Adams, Jr. and Associates. (Washington, D.C.: The Brookings Institution, 1977. Pp. ix + 268. $9.95, cloth; $3.95, paper.)

The most ambitious of a recent spate of books, articles, and government reports on revenue sharing is a Brookings Institution study under the tutelage of Richard P. Nathan. Shortly after the enactment of the State and Local Fiscal Assistance Act of 1972, the first revenue sharing legislation, Nathan and his colleagues undertook an extensive study to monitor, over time, the program's fiscal, structural, and political impacts on 65 jurisdictions: 8 state governments, 29 cities, 21 counties, 6 towns, and one Indian tribe. Three volumes are planned to disseminate their findings. *Revenue*, the interim report, is best understood and appreciated when read as a compansion to *Monitoring Revenue Sharing* by Nathan, Allen D. Manvel, Susannah E. Calkins, and associates published by Brookings in 1975. The same will be true of the third, scheduled for 1979, since among other things it will interrelate data from the first two volumes.

This book, nevertheless, stands alone as an analysis of the first major federal noncategorical grant program. The authors not only review congressional issues and debate on the 1976 renewal legislation and update their first volume's findings on the fiscal and political consequences of the program for recipient jurisdictions; they also add a chapter on a timely subject—the impact of revenue sharing on central cities—and then look ahead to future issues Congress will undoubtedly face when the program is again considered for renewal. Their data are derived mainly from the survey reports of 23 field associates located in or near the sample jurisdictions. This reliance is far more extensive in *Revenue* than in *Monitoring*. Some may conclude that too much of the analysis rests on the expertise of the associates. This is not a problem, because the authors do a commendable job of providing a frame of reference within which the associates must work as well as instructing them in data collection and reporting criteria. Furthermore, the survey data are supplemented with and supported by aggregate statistical data derived from government reports, U.S. Census information and other research. Nathan and Adams argue that this approach

> provides an important addition to social science research methodology. It is not a substitute for traditional statistical approaches, but rather a needed supplement, providing a means for analyzing new programs at an early stage, often the most volatile period in terms of administrative and legislative changes. Moreover, only through a network of informed field observers . . . can important political and structural factors be identified and related to the fiscal impact of revenue sharing (pp. 28–29).

Federal program objectives are molded by the diversities of states and their localities. Thus, not unexpectedly, the revenue sharing program is having mixed consequences: some hoped-for and anticipated and other unintended. The 1972 act made these results inevitable, and in the first chapter we learn that "on the principal issues—permanent funding, state and local flexibility, and the distribution formula—the renewal legislation closely resembles the 1972 act" (p. 23). With revenue sharing, like most programs, policy issues are never finalized, rather they are massaged periodically until the ache returns at a later time. When the present revenue sharing legislation ends in 1979, questions left unanswered in 1976 will again have to be aired: Should states and limited function governments be excluded from the program? Has the "fungibility" issue been adequately dealt with in procedures pertaining to nondiscrimination, public participation, and in auditing and accounting requirements?

The authors also assist us in understanding a number of complex factors which influence the program objectives of revenue sharing. While it generally provides "something for everybody," its benefits are not uniform: some redistribution of shared revenues is occurring. For example, central cities are disportionately benefitted by the distribution formula. Limited tax stabilization is resulting, especially for hard-pressed cities. Counties are utilizing their funds more than other local governments to expand service deliveries. Furthermore, the authors find some decentralization of policy responsibilities and a modest restructuring of decision making from specialists to generalists. As the program matures, we also learn that state and local officials are expressing more confidence in its continuance. This is shown by the number of jurisdictions that have now merged shared revenues into their regular budgets, and by the reduction in spending for capital improvements. Even though revenue sharing is rendering these changes, the major conclusion the authors emphasize is that they are *far from dramatic.*

As an interim report, *Revenue* accomplishes its purpose. It provides a storehouse of conclusions and suggestive hypotheses about fiscal federalism and intergovernmental relations for a wide audience of social scientists. Practitioners will also find this book useful, because it addresses a major public policy issue according to established criteria in a methodologically sophisticated fashion. While their prescriptions may not provide the "best" answers to future issues, the authors offer a starting point for debate. In these terms, this is unquestionably an important book.

ROBERT D. THOMAS

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Congress Oversees the Bureaucracy: Studies in Legislative Supervision. By Morris S. Ogul. (Pittsburgh, Pa.: University of Pittsburgh Press, 1976. Pp. xii + 237. $9.95.)

Morris Ogul has written a very stimulating book on the important but neglected subject of congressional oversight. The book is based on 89 interviews conducted in 1965 and 1966 with congressional personnel, interest group representatives, and high-level bureaucrats. Case studies of the oversight behavior of the House Post Office and Civil Service Committee and subcommittees of the House Judiciary and Government Operations Committees form the substantive base for the analysis which is keyed to an analytic scheme which Ogul developed. The major questions explored are: "Why is it that the Congress acts as it does in its efforts to oversee? And what does the Congress accomplish by its efforts?" (p. 4)

One of the most difficult tasks facing the analyst of congressional oversight lies in defining the phenomenon. Reacting against narrow conceptions of oversight, Ogul has developed the following working definition: "Legislative oversight is behavior by legislators and their staffs, individually or collectively, which results in an impact, intended or not, on bureaucratic behavior" (p. 11). The purpose of the definition is to broaden the study of oversight beyond a few specified techniques such as investigations and to blur the conventional rigid distinction between legislation and oversight in order to "seek out oversight behavior wherever performed throughout the legislative process" (p. 11). This is a worthwhile goal, as demonstrated by Ogul's sophisticated treatment of what he terms "latent oversight," and a useful corrective to past work. However, Ogul's definition is so broad that it is hard to exclude very many activities from inclusion under the oversight rubric. Fortunately, Ogul's cases and discussion implicitly accept a somewhat narrower conception of oversight–review of the policies or actions of the executive branch–while wisely avoiding a narrow focus on particular techniques or settings for oversight behavior.

Ogul's scheme for understanding oversight places politics at center stage. He lists seven opportunity (conditioning) factors which "enhance or lessen the potential for oversight irrespective of any single, concrete situation" (p. 4). The factors are legal authority, staff resources, subject matter, committee structure, status on a committee, relations with the executive branch, and member priorities. While some of the opportunity factors directly involve member attitudes, all are described in terms of the propensity of members to utilize the opportunities they present. For example, in discussing staff resources, Ogul is quick to pair "access to staff *and* the willingness to use it" as "important preconditions to substantial oversight" (p. 13, emphasis mine).

An "oversight-maximizing syndrome" (p. 21) is said to exist when favorable aspects of the factors occur simultaneously, but the proclivity to oversee is likely to be turned into actual behavior by conversion factors (defined as "the most common situations in which propensities are converted into behavior" [p. 227]). Two basic conversion factors are identified: (1) shared disagreement with a program being implemented; and (2) external events such as a scandal or a sudden crisis. The opportunity and conversion factors enumerated are useful aids in the analysis, although it is not always clear how distinct the factors are. For example, it is hard to distinguish the "visibility of the issue," which Ogul mentions in discussing the subject matter opportunity factor, from a sudden crisis or scandal, which are given as examples of the external events conversion factor. An additional problem is that in Ogul's case studies the conversion factors do not appear to be nearly as important as the analytical scheme would lead one to expect. Member priorities appear to dominate with or without especially favorable conversion factors, although once again it is difficult to disentangle member priorities from the conversion factors.

The case study data developed are quite

interesting. The studies themselves are clearly the work of a skilled interviewer and sensitive observer of the congressional scene. Ogul is suitably cautious in stating generalizations based on the case materials and he carefully reminds the reader that his data come from a limited period and a small number of committees (pp. 192–93). The criteria for selecting the cases are also clearly stated. As Ogul notes, however, "in each instance the criteria for selection were somewhat different" (p. 24), and one wonders why a more carefully controlled case design was not employed.

Two aspects of Ogul's analysis are particularly worthy of note. The first, mentioned briefly above, is his treatment of latent oversight. Ogul looks to legislative hearings, casework, and reports required by legislation for evidence of "oversight activity [that] has taken place during legislative activities not labeled oversight or commonly associated with it" (p. 180). The evidence he marshals indicates that this can be a fruitful area of inquiry, although it is clear that the amount of latent oversight uncovered by Ogul in the years and areas he studied was not enough to change our basic understanding of the amount of oversight done and that "adding latent to manifest oversight leaves unaltered the intermittent and noncomprehensive character of the performance of the function" (p. 180). Still, the introduction of the concept will stimulate scholars working in the area to consider a broad range of congressional activities in their research and ultimately give us a more complete understanding of oversight.

The second aspect I wish to highlight is, in my view, especially important. Ogul is interested in finding out what Congress accomplishes by its oversight efforts. His case study data indicate that "specific oversight efforts such as that of the SSIP [Special Subcommittee on the Invasion of Privacy] on the data bank issue can have a substantial impact. Overall, [however], congressional influence tends to be more scattered and slight" mainly because Congress members, at the time of the study, were generally not motivated to perform much oversight and certainly not continuous, comprehensive, and systematic oversight. The major impact of oversight seemed to be to "alert executive agencies to problems and reexamination more than to provide comprehensive and systematic direction" (p. 185). Much difficult work remains to be done in specifying the conditions under which congressional oversight is likely to have an impact on the administrative agencies and the nature of that impact, but ultimately this is why we are interested in oversight. Ogul points us in this direction and others, hopefully, will follow his lead.

JOEL D. ABERBACH

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The Gray Lobby. By Henry J. Pratt. (Chicago: University of Chicago Press, 1976. Pp. x + 250. $15.00.)

The Gray Lobby is a study of the growth of what Henry J. Pratt terms "the senior movement" and of the role that movement has played in old-age policy enactments. Beginning his analysis in the 1920s, Pratt argues that the rise of old-age groups is at best interpreted in social movement terms. In keeping with this perspective, he details the growth of old-age organizations from their "popular stages" in the 1930s and 1950s to their present state of "formal organizations." In their current state, Pratt sees the old-age organizations as having been able to frame issues, get them onto the political agenda, provide supportive technical information, and prod legislators into favorable actions. Out of these efforts has developed an "old-age policy subsystem" in which the old-age organizations represent the non-governmental constituency dealing with age-related congressional committees and executive agencies.

Pratt's strongest material is that dealing with the rise and organization of contemporary old-age groups. The only book-length treatment of national old-age groups published in over 20 years, *The Gray Lobby* provides a thorough account of the "senior citizens for Kennedy" movement (chapter 5, with Peter A. Corning), the genesis and development of the principal old-age interest groups, and an analysis of their internal structures and resources. His description of the current old-age policy subsystem is also useful, although some of his specific assertions are open to question (e.g., whether one can appropriately consider the House Ways and Means and Senate Finance Committees as part of an "income and health maintenance" subsystem, given the symbiotic and cooptative relationships to which the subsystems teminology usually refers).

Pratt's larger analysis contains two more substantial problems: (1) whether the influence of senior citizen organizations is principally the result of a social movement and (2) the extent to which recent policy enactments can be attributed to these organizations, however they may be best understood.

To the extent that "social movement" connotes the mobilization of large publics for purposes of social action, it is not clear that the current configuration of old-age groups is best understood in such terms. Pratt acknowledges that the "random and erratic" behavior and the "panic and impulsiveness" posited by his two social movement sources have not been present among the aging. The "modern senior movement" born in the 1950s, in Pratt's analysis, appears to consist of the following: three or four individuals prominently associated with old-age problems; the growth of senior centers incorporating only "1–3 percent" of "eligible individuals" (p. 43) and in which overt political activity was discouraged; an "ideology" based on a well-defined sense of grievance but which may have consisted of "no more than a small minority of the [old-age population]" (pp. 49–50); and the establishment of the American Association of Retired Persons and a retiree department in the UAW. The movement in its current phase consists principally of AARP and other mass-based organizations, their memberships numbering well into the millions.

A counter-interpretation of organizational growth in aging would suggest that older persons were being organized rather than organizing and to a degree that places the social movement thesis in severe jeopardy. Developments in the 1950s revolved around a small number of activists, and the subsequent growth in organizational memberships was probably more the result of "side-payments" and divisible material incentives (insurance coverage, cut-rate drugs, travel packages) than collective policy concerns. Pratt's chapter which attempts to dismiss this alternative interpretation is not convincing.

The difficulties associated with the social movement thesis are most apparent where Pratt's discussion turns to the factors associated with various old-age policy enactments. His concern with the role of the movement leads him to overweight the effect of popular "demands" and organizational "pressures" and correspondingly to underemphasize the agendas and latitude of actual decision makers and the larger contexts in which old-age policy decisions were being made. Thus, Pratt discusses the Social Security Amendments of 1950–which increased OASI benefits by 77.5 percent and brought self-employed persons into the system–under a section entitled "A Minimally Adequate Statute," presumably because at the time the "senior movement" was nowhere to be found. In contrast, he gives much weight to a number of more visible but smaller advances that took place in the wake of a White House Conference on Aging, an event in which organized interests were prominently featured.

The suggestion that these latter developments were directly related to the White House conference is also subject to question. While the conference's recommendations and the ensuing enactments were remarkably similar, there may have been more of a spurious element present than Pratt allows. Looking in turn at the legislative actions Pratt discusses, the Older Americans Act Amendments were necessitated by the president's promise at the conference to increase appropriations under the act, a gesture better understood in symbolic rather than "influence" terms, given the small amount of money ($55 million) in question. The social security increases of 1972 were part of a much larger executive/legislative struggle in which social security may have been more of an expensive pawn than a central issue. The enactment of a nutrition program for the elderly stemmed primarily from pressures of service providers and the interest of Senator Edward Kennedy to expand earlier demonstration projects. Pratt's approach, which relies heavily on interviews with the organizational actors involved and is frequently hedged with side comments about "benign environments" and "favorable conditions," does not systematically address these other kinds of explanatory options and thus leaves the reader with no firm base from which to evaluate the merits of the case.

Finally, Pratt's approach leads him to omit almost entirely two pieces of legislation which have been critical to the well-being of over 10 percent of the older population: the Old Age Assistance title of the original Social Security Act and its 1974 successor, the Supplemental Security Income program. In that aging-based groups have not had great interest in public assistance programs for older persons, this omission is fully appropriate; however, because Pratt contends that he has focused "attention on particular [legislative] developments believed to be representative of the larger whole" (p. 7), the omission is misleading.

In conclusion, Pratt's account of the growth, structure, and policy concerns of mass-based aging interest groups is very informative, and a valuable resource for the reader wanting a thorough and updated assessment of these groups. However, for the reader interested in understanding what has been responsible for the increased legislative authorizations and appropriations on behalf of older persons, Pratt's book provides only a somewhat narrow and incomplete picture.

ROBERT B. HUDSON

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The Insoluble Problems of Crime. By Robert P. Rhodes. (New York: John Wiley, 1977. Pp. ix + 269. $7.95, paper.)

This work assumes that criminal activity "is likely to remain relatively high in the United States in the foreseeable future" regardless of the wisdom or unwisdom of public crime policies (p. 257). From the point of view of reformist ideologues, Robert Rhodes will be viewed as a conservative. But the matter is not that simple and the author cannot be neatly pigeonholed.

In spite of caution and ambivalence, Rhodes is far from defeatist. His pessimistic assessment is a Cartesian method of inquiry, clearing the mind of ideological clutter so that concrete and practical measures may be examined on the basis of good data. The search for basic "solutions" can actually be an obstacle to the modest and proximate improvements which should receive our attention and effort, however modest the promised results.

The heart of the dilemma of criminal justice is that all interest groups exploit the facts of crime as a way of rationalizing and promoting their own ideological commitments and values. Rhodes argues that "conflicts of interest in American society and lack of awareness of the multidimensional nature" of crime prevent discussion of "feasible policy alternatives to reduce property and violent crime that are consistent with current standards of due process and a humanistic correctional system" (p. 3). Not only are we unable to agree on solutions, but we also cannot agree on the nature of the problems.

The device of Cartesian doubt is effective in making a case for incremental improvements in the system. Rhodes may be charged with undue optimism about the usefulness of policy analysis. Fortified with data charts and computer print-outs, Rhodes asserts that scholarship can become "non-partisan expertise when data supports a policy consistent with achieving objectives for which there is no disagreement" (p. 5). Where such conditions obtain, policy studies can contribute slight, but nonetheless real, amelioration; the creation of "political consensus on objectives" will eliminate the element of political chance and replace it with scientific (not the word Nieburg uses, but rather "realistic and feasible") public policies.

Nieburg's optimism about policy analysis contradicts the basic ambivalence of his study, leaving the reader with a sense of disquietude. Whatever one's own commitments, the reader will be dismayed and agitated by witnessing the attack on myths and preconceptions; not merely upon the old and rejected, but even the fashionable, liberal, and innovative. The thoroughness of the dismantling process deserves the highest kind of praise and locks the reader's fascinated attention to every page of charts and diagrams.

This work is intelligent, provocative, literate and imaginative, while building its arguments on large juicy hunks of empirical data. In many ways, this work represents the best kind of policy analysis and could serve as a model for inquiries which seek to unite insight, data, values, and criticism into resource documents for public dialogue.

Rhodes has provided an expanded and data-buttressed version of James Q. Wilson's *Thinking About Crime* (New York: Basic Books, 1975). Taking on such works as Ramsey Clark's *Crime in America* (New York: Simon and Schuster, 1970), and radical right advocates of police power, Rhodes also batters the behavior modifiers, the electronic gadgeteers, and his fellow incrementalists.

This work is essentially troubled, cautious, and ambivalent—attempting to separate ideological analyses of crime ("the permanent hidden agenda of criminal justice": Walter B. Miller), from proximate causes and remedies in order to free for evaluation the area of public policy which is responsive to budgetary and institutional choices.

The author writes with feeling for the language, for the classics, for good turns of speech. To be a stylist while writing with clarity is an achievement in its own right. That Rhodes can transmit his own troubled vision without slighting the data is a great achievement.

He argues that for the most part "crime causation is not central to crime policy" (p. 260). Given the present state of knowledge, policies cannot be based upon dubious theories of background causes, but must address proximate and superficial relationships where decision making can have genuine effects. People are being victimized now and "practical policy questions cannot wait for a wholesale alteration of the social structure to resolve the problems of crime." The touchstone of analysis is "victim suffering" which Rhodes hopes will become "the standard measure of public policy in reducing crime in the future" (p. 260). Yet, even if this is the case, and the conscientious marshalling of empirical data is brought to bear, "the impact of an effective crime policy will still be limited." But at least we may avoid simplistic crusading zeal of special interest and ideological arguments. Crusades, he points out, produce either "pyrrhic victories or dangerous frustration" (p. 261).

This work is a paean to incrementalism and caution in crime policy. It is a hymn to ambiguity, ambivalence, and self-doubt. It is a conscientious attempt to array the data and review the arguments of all interested parties from a Hamlet-like stance of uncertainty and humanistic insight. Searching and critical, this study deserves the attention of every political scientist.

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Managing Presidential Objectives. By Richard Rose. (New York: Free Press, 1976. Pp. x + 180. $12.95.)

"Do governments have objectives?" This question begins Richard Rose's splendidly written study of the implementation of the Management by Objectives system introduced to the federal government during the aborted second Nixon administration and continued under the brief presidency of Gerald Ford. The author's answer to this disarming but entangling question is yes, but these objectives are multiple, often contradictory, frequently derived from prior commitments, and never fully comprehended across the balkanized agencies and operating bureaus that collectively comprise the executive branch. It is not that government has no objectives, Rose argues, "but rather that it has *too many* objectives to be comprehended or integrated by a single mind or institution within government" (p. 6).

The question, "Do governments have objectives?" is readily transformed into a related question, namely, can a president effectively gain control over his administration in order to pursue *his* objectives? How one approaches this question depends largely upon one's willingness to grant that presidents have proprietary rights over the executive branch. Steeped in the tenets of rationality, coherent purpose, and executive coordination, the prescriptions of management science typically have been based upon a willingness to grant that assumption. However, because none of the framers of the Constitution attended Harvard Business School, the prescriptions of management science for "governing better" often have foundered on the hard shoals of the fragmented institutional realities inherent, if not fully sketched, in the framers' designs.

No matter how much control presidents wish to exert, there are stringent limits to how much they can exert. Distance, complexity, past commitments, finite amounts of political capital, and a need to attend to the necessities of office—all act to constrain presidential control over the executive branch. As Rose sees it, the growth in presidential power stems from a rising minimum standard of expectations rather than from a growth in presidential capabilities for maximum achievement. The operations of government go on nonetheless, largely through the inertia generated by prior commitments, habits, and the force field of interests surrounding the management of diverse programs. Presidents and the bureaucrats who manage these programs frequently have different objectives and rarely do they share the same perspectives. Presidents want to know how they can get people in the agencies to do what they want, whereas bureaucrats want to know how they can get presidents to state objectives that have any operational meaning. One seeks to control while the other seeks directions in terms "less grandiose than those of Mission Impossible" (p. 32). To the extent that operationally meaningful directives are absent or go unmonitored, the power of discretion lies with the bureaucrats.

Presidential activism whets the appetite for presidential control and most recent presidents have been inclined toward an activist conception of their role. Presidents' desires being what they are and Washington being what it is, there is no shortage of technocrats selling schemes to create unity of purpose in the executive branch. Policy science and management science are to Washington what the fashion industry is to Paris. The prescriptions of the former are usually as ephemeral as the designs of the latter, notwithstanding the ultimate durability of either industry. Moved by a dual penchant for budgetary constraint and for managerial control over a permanent government it viewed with suspicion, the Nixon administration, spearheaded by Fred Malek and Roy Ash at OMB, introduced the Management by Objectives system.

MbO's purpose was to establish departmental objectives and define operational milestones for meeting these objectives within *existing* budgetary constraints. On a grander scale, its objective was "to strengthen the President's direction of the federal executive" (p. 58). In operation, Rose shows, departments tended to submit objectives that were quantifiable, immediate, definitive, and precise. The single largest category of objectives, for instance, dealt with matters of internal department management. Agencies with a clear engineering or administrative sense of what they were about found the system most workable. Others, dependent upon interaction with not clearly controllable forces, were least apt to institutionalize the system in any meaningful way. In practice, MbO promoted effective (chiefly intra-departmental) monitoring when there was consensus about objectives and relative clarity in the techniques available for attaining them. As such, its influence was "consistent with consensual goals of good management" (p. 137).

How, then, does MbO enable presidents to get a handle on the executive branch? The answer basically is that it doesn't or, at least, it didn't. Although MbO was intended to advance presidential objectives, Rose claims that the White House was not a significant client for the information produced by the system. Political conditions undoubtedly had some bearing on this. For by the time MbO got rolling, the Nixon White House staff was consumed in the miasma of Watergate and had lost whatever ability it might have had to influence greatly what was happening in the agencies. Nixon's successor, Gerald Ford, perhaps in the light of Watergate, displayed little ambition to impose stark direction from the White House. But more fundamentally, MbO in practice was a system best suited for in-house evaluations of departmental routines. By helping to monitor progress toward routine objectives, it more effectively served assistant secretaries than it did the White House. It is dubious that "better management" in this limited sense is what presidents or their political advisors have in mind when they talk about controlling what they usually assume is "their" administration.

Left unanswered, mainly because it is unanswerable by empirical analysis alone, is the question of what it is that presidents ought to be in control of. The constitutional design has not been generous to the claims of management science. Its virtues, as Rose views it, lie in the accommodation of multiple and contradictory objectives. Perhaps. But one must ponder whether rampant institutional fragmentation really does reconcile diversity or whether it runs what Rose in another context asserts as "the risk of pursuing activities that are milestones on the road to nowhere" (p. 130). Rose's analysis does not answer that question, but it illuminates brilliantly the limitations of technique for enhancing presidential control over those matters that presidents most want to control.

Written with grace and analytic sensitivity, this book deserves far more attention than its publishers have seen fit to give it.

BERT A. ROCKMAN

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Media, Politics, and Democracy. By Bernard Rubin. (New York: Oxford University Press, 1977. Pp. xvii + 192. $8.00, cloth; $4.00, paper.)

Organized in five short chapters, *Media, Politics, and Democracy* is devoted to discussing a series of problems in contemporary American society that relate to the mass media, especially to the news media. According to the author, the purpose of the book is to take a look at suggestions "for improving *the system,* emphasizing roles of the media" (p. xvii). Each chapter is richly illustrated with an array of anecdotes and case analyses. Each chapter contains a variety of normative conclusions and speculations, and organized sets of more specific topics linked to the central theme of the discussion. It is possible to note only some of these topics here.

Rubin discusses a number of problems in objective communication of politically significant events and mechanisms for maintaining media fairness and accuracy in the first chapter. He heaps laurels on the National News Council, a private group formed in 1973 to review the work of the major news suppliers in the face of opposition from news interests. Harsh criticism is reserved for style and content of news that treats events as "theatrical occurrences" and fails to play down the sensational in relation to more important aspects of events that help people understand important social processes. The author argues that the press ought to play an active role in influencing public opinion about important events by assembling conflicting aspects of events, maintaining public interest in issues, serving as a catalyst for opinion, and providing a link between elite views and general opinion. FCC criteria of public service, balancing local and network programming and balancing public affairs and other programming, are concluded to be too weak. They do not take problems with advertisers, program production, and mass audiences into account adequately. Rubin argues that media managers should increase coverage of anatomical and biological aspects of community life (p. 19) and issues of universal concern, such as the environment, ecology, and energy (p. 21).

Chapter 2 focuses on media content and its implications for popular values. Asserting that democratic-minded media managers ought to use the mass media to help people lead more socially rewarding lives, Rubin sees much media content as misdirected, and as turning society away from democracy through illusion mongering, perpetration of ignorance, and acceptance of brutality (p. 44). In the treatment of violence on television, for instance, more adequate portrayals in ways meaningful to audiences are needed, rather than the complete sanitization of programming by fostering illusions of a society in which violence is not present. The media ought to produce a greater number of "rational documentaries." Mass media, Rubin argues, can best play a role in national development when new material is employed as part of a broader policy context. He believes that the media will destroy democracy if television is seen only in terms of balancing good and bad. Rather, the media should be used to stimulate learning activity. Simple entertainment fare falls far short of these goals. Indeed, Rubin asserts that the entrepreneurial model of television management must be challenged effectively to reach this goal.

The managers and employees of the mass media also must learn to criticize themselves more effectively and to perform more responsibly in a free society. The press must not allow leaders "to avoid the glare of publicity" (p. 65). Rubin asserts that the press has not been completely adequate in this regard, since the biggest stories are not adequately covered: coverage shortchanges processes of government, local stations are not adequately staffed to develop news stories, and there is little enthusiasm for documentary programming. Such concerns are all the more important, since Rubin sees the press as the only efficient instrument to keep the government honest (p. 87). Problems of governmental manipulation of the press are illustrated by examines drawn from the Kennedy and Nixon administrations.

Chapter 4 describes governmental control of information and constraints on that control, such as freedom of information legislation, shield laws to protect reporters from being required to identify their sources, and access to classified information. Rubin asserts that the government has been tampering with press freedoms to the point that there is a trend toward autocracy (p. 117). Using Watergate and Vietnam as examples, the author asserts that the Nixon administration manufactured distortions as "products of administrative routine" and that news coverage was not adequate to help people analyze what was going on behind the events (p. 119).

Chapter 5 summarizes several aspects of recent campaign reform acts. Rubin debits the electronic media with increasing public apathy and decreasing voting turnout because they emphasize the spectacular in political campaigns (p. 144). But he also credits the media with being as influential as the articles of impeachment in "bringing Nixon down" (p. 150). Mentioning the roles of political consultants, the author credits media with changing patterns of partisanship. He also goes on to assert that democratic beliefs have been "dangerously diluted and exchanged for short-term political preferences induced by television's political commercials" (p. 166). But Rubin also asserts one page later that "*dirty politics,* aided by mass media, is the worst problem confronting us."

Two types of concerns characterize writing about the media and politics. One is a concern for normative issues and speculative interpretations of broad social trends. The second concern is that of the behavioral scientist who applies the techniques of science to explain aspects of communication processes in political contexts. *Media, Politics, and Democracy* is clearly in the former tradition. Like so many other works, it suffers from inattention to knowledge in the latter area. Rubin repeatedly makes strong statements about specific media processes and effects with little qualification. He does not include findings of rigorous empirical studies which might offer a tempering influence on such assertions nor does he cite them in most instances. I would have found the book much easier to read and more effective in communicating ideas if the various topics addressed had been related in a more explicit fashion.

C. RICHARD HOFSTETTER

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The Unmaking of a President: Lyndon Johnson and Vietnam. By Herbert Y. Schandler. (Princeton, N.J.: Princeton University Press, 1977. Pp. xx + 419. $16.50.)

This book is a rather detailed assessment of the currents of opinion and argumentation over the war in Vietnam as they changed and developed in and around the White House in the weeks preceding March 31, 1968 when Lyndon Johnson announced he would not stand for reelection.

Originally written as a Harvard dissertation, Herbert Schandler's work draws on a number of sources: the voluminous literature that has already been published on this period in American diplomatic history (the bibliography is superb), his own experience and research as a staff officer in the Pentagon, and interviews with over 20 of those involved with the 1968 decisions (many of whom have themselves published accounts of the period). He was unable to interview Johnson or former defense secretary Robert McNamara, but he did talk to most of the other central personalities. The book also includes a brief but informative discussion of U.S. policies and perspectives from the 1965 decision to intervene until the trauma of the Tet Offensive of early 1968.

Although Schandler does not come up with a great many new insights, he has some original and provocative things to say and he does an intelligent job of sorting through the material and of presenting it in a coherent and quite dramatic fashion. One gets a good sense of the shifting perspectives of the war and for the strands of argument and policy promotion that went into the March 1968 decision. The decision makers and what influenced their decisions are not psychoanalyzed, but their points of view and the way they affected the decision-making process are carefully described and soberly evaluated.

Interestingly, although Schandler seems to see the March 31 speech as the culmination of all the pre-Tet doubts and the post-Tet frustrations, it could be argued that the speech actually had very little relevance to the really important changes in Vietnam policy that took place at the time. For, as Schandler makes clear, the major policy shift involved two interrelated points: (1) there would henceforth be a ceiling on the number of U.S. troops committed to the war, and (2) the South Vietnamese would be expected to take over an increasingly greater share of the fighting. This latter policy was to pick up the name "Vietnamization" and, rather remarkably, it has often been credited to Richard Nixon.

Both points had essentially been adopted at least two weeks before the president's dramatic speech (p. 229). They were part of the administration's rather consensual decision to turn down the military's request to increase U.S. forces by some 206,000 men. (Schandler's accounting of the curious way this request came about is excellent.)

Although Schandler sees these two policy points in Johnson's speech (p. 290), the first was only fragmentarily and very indirectly mentioned in it (pp. 284–85) and the second was not really there at all. Instead, the major points of the speech were an announcement that the bombing of North Vietnam would be partially halted (based on a proposal half-heartedly proffered by Secretary of State Dean Rusk) and a declaration that the president would not run again.

What came out of Johnson's speech was not a change in U.S. policy in Vietnam but rather the opening of peace talks. The North Vietnamese rather unexpectedly shifted their policy (though not their war effort) by agreeing to "preliminary" discussions even though the U.S. had not said it would stop the bombing unconditionally as the Communists had previously demanded.

Schandler says these talks led to peace (pp. 289, 320). But it seems rather that they were mostly a vacuous, headline-dominating charade. Five years later they culminated in an agreement involving prisoners and some temporary promises, but long before then American participation in the fighting—particularly the ground fighting—had been "Vietnamized" nearly to nothing. In terms of U.S. combat losses, a kind of "peace" had already been obtained. And "peace" for the Vietnamese, of course, did not come until 1975, seven years after Johnson's address, two years after the "peace agreement" was concluded.

Schandler's analysis can also be used to suggest that Defense Secretary Clark Clifford's contribution to the 1968 policy decisions may have been somewhat different from what is usually supposed. Clifford did a lot of unspecific moaning in high places about how the war had grown too costly and this may have contributed to the atmosphere that produced the policy change—Schandler certainly thinks it did. But others felt this way too, and the troop limitation (or the rejection of the 206,000-man request) was largely dictated by economics, by negative attitudes toward reserve mobilization, by seeming improvement in South Vietnamese fighting ability, and—in any event—by domestic political realities in the aftermath of Tet. While Clifford contributed little to the two major specifics in the March 31 speech, he did apparently help to give it a dovish tone and Schandler suggests (but has no confirming evidence) that this may have made it easier for the Communists to agree to talks.

But Clifford's major contribution to the 1968 decisions may have been made *after* Johnson's speech. In statements and press conferences in the following months, the secretary firmly nailed down the troop limitation and Vietnamization policies perhaps specifying things more than Johnson really wanted and, to a degree, making them irrevocable. Schandler flatly declares (p. 328) that Johnson could allow this because the peace talks were on, but neither argument nor evidence are given for this conclusion.

While Schandler's book ably details many aspects of the decision-making process in early 1968, it tends to be a bit weak on domestic political considerations. There is a brief chapter partly devoted to the candidacies of Eugene

McCarthy and Robert Kennedy, but relatively little is done to link these specific considerations to Johnson's decision making. It is possible, of course, that Johnson, the supreme politician, actually managed to divorce from his thinking what was going on in the primaries. If so, that would be a remarkable phenomenon worthy of consideration in its own right. It might be observed, however, that Johnson's speech came just a couple of days before a primary in Wisconsin in which he was expected to do quite badly. Schandler hardly even suggests that this might have affected the timing or the content of the speech (p. 270). And in a couple of places (pp. 302, 347) he almost seems to suggest that Johnson's decision to withdraw was successful in unifying the country on the Vietnam issue.

This book, then, is not the complete, nor will it be the last, word in presidential decision making in the dramatic early months of 1968. But by bringing together so much material and arranging it so carefully and often perceptively, Schandler has made a valuable contribution.

JOHN E. MUELLER

University of Rochester

The Politics of Education: The Seventy-sixth Yearbook of the National Society for the Study of Education—Part II. Edited by Jay D. Scribner. (Chicago: University of Chicago Press, 1977. Pp. 367. $12.00.)

For 78 years the National Society for the Study of Education has been issuing yearbooks in areas of interest to its members. For the first time, the politics of education has been introduced as a yearbook topic. The admission is long overdue that there is politics in education, and that the subject is worthy of study as a new field of disciplined inquiry. The volume reviewed here is the second of the 1977 issues; the first was devoted to the teaching of English, a more typical topic for the society.

This work, however, may be mistitled. It should have been called "The Politics of Secondary Education"; there are no contributions from the growing literature on the politics of higher education. Although some of the material rehashes information appearing elsewhere, each chapter contributes to the whole.

This volume contains 11 chapters, written by a collection of political scientists and educators. The political scientists (Hawley, Wirt, Ziegler, and associates) include the leading specialists in the field. Because they are so few in number, contributors make frequent references to each others' works.

The quality of the papers is uneven, as with most collections. The chapters are classified into five sections: an overview of the field; concepts and methodologies; major influence systems at the national, state, and local levels of government, including the interrelations of those systems as they influence educational policy; decision making, communication, change, and evaluation; and, in conclusion, an examination of directions which future study of the politics of education might take.

In many ways the concluding chapter, by Willis Hawley, is the most provocative; it asks the kind of hard questions that need to be raised. As usual, Hawley picks a catchy title: "If Schools are for Learning, the Study of the Politics of Education is Just Beginning." The editor describes the chapter as the "so what?" contribution. After reviewing the general issues of policy studies, Hawley applies these issues to the politics of education field. He says:

> It is not my goal to debunk or devalue the work of the growing number of persons who have decided that schooling is a worthy subject for political analysis. It is instead to urge that we take seriously the notion that the purposes of politics are to resolve conflict and to alter or sustain the conditions that make up the life experiences that people value. If that is the purpose of politics, its study must go beyond processes and formal policies to examine the linkage between how political actors (both public officials and other citizens) behave and the factors shaping such behavior to studies of the consequences of the delivery of public services for those the services are meant to affect (p. 326).

Hawley goes on to raise a series of questions that he thinks should be asked by students of the politics of education. Under the category *governance*, he asks, "Does community control or greater parental involvement affect the values children hold or their attitude toward school and achievement?" Under organizational change, are "the linkages between the formal political process and the teacher . . . so attenuated that there is no significant relationship between what we usually think of as political events and the experiences students have in schools?" Under political learning, "Does school desegregation increase interracial tolerance and understanding?"

There are many more provocative questions mentioned that should be addressed. Why have they not been studied? Hawley gives three reasons:

> First, and most obviously, to engage the ques-

> tion "so what" is to take on a whole set of assessment problems, the answers to which may require patience, energy, and skills many of us do not have.... A second general problem is that most of us, while we recognize that important political decisions are made outside formal political institutions ... feel uncomfortable and uncertain in trying to identify these decisions, much less in assessing their consequences.... Third, and more speculatively, "modern" political science is rooted in earlier commitments to research and writing that sought to compare what is, at least implicitly, to what should be with the rescriptions derived from normative political and organization theory (p. 332–33).

Hawley takes a parting shot at his colleagues: "Justifications for research and writing about politics that are couched in such terms as 'contributions to the discipline' or, 'theoretical or methodological rigor,' while they have some place, might be compared more often to criteria like the extent to which we advance the probability that greater numbers of people will experience justice, social equality, opportunities for self-expression or self-actualization, or a higher standard of living."

The yearbook would have been a better volume if Hawley's chapter had come first and the other authors had responded to his questions, instead of being concerned with such topics as "Methods and Conceptualization of Political Power in Education."

SAMUEL K. GOVE

University of Illinois

Unchallenged Violence: An American Ordeal. By Robert Brent Toplin. (Westport, Conn. and London: Greenwood Press, 1975. Pp. 332. $15.00.)

Written by a professor of history at Denison University, this work is a broad and sweeping review of the problems associated with violence in the United States. It asks and attempts to answer such fundamental questions as: What causes violence? What are its consequences? And what can be done about the problem?

The book is divided into three sections: problems, sources and conclusions. Actually, the categorizations, like the problem area itself, deny neat classifications. Violence seems to permeate most aspects and areas of American life and this becomes clear in Toplin's treatment. The first sections deal with such manifestations of violence as the pervasive fear it engenders, crime, law and order, youth, racial clashes, protests, assassinations and types of sanctions. The section on sources reviews aggression and machismo explanations, violence as a cultural ethos, the availability of weapons and especially guns, the communications media as culprit, and so on.

The author obviously has chosen to take an inclusive perspective. Individual chapters could serve as bases for entire books on their own. Toplin ties the varied subject matter together into a broad thematic context that emphasizes America's fatalistic acceptance of high levels of cultural violence, a fatalism that, as he sees it, tends to deemphasize attempts to deal realistically with the problems raised.

Toplin sketches the reaction from the consensus school of historians who looked at the United States as something decidedly special and blessed, to a vision of America, following in the wake of the assassinations of the sixties, the cities on fire, Vietnam, racial conflicts and street demonstrations, as a sick society, a nation deeply in trouble. The emphasis now appears to be on the United States as a nation spiritually exhausted and accepting violence as a cultural tradition, endemic to the country and irremediable. Of theories that argue violence is innate in the American character, Toplin writes: "Such explanations lead straight to the most debilitating implications of accepting an 'apple pie' thesis as the last word on violence. When people believe violence is the 'American way' and nothing much can change it, they stand immobile in the face of challenges and resigned to accepting their fate. They prepare their minds to live with violence instead of fighting it" (p. 275).

Toplin has no sympathy with such a posture. He is also critical of the social scientists who offer the evidence from their research on, for example, whether models of violent behavior (say, as seen on television) can stimulate others to act in a violent manner but refuse to go beyond their own findings to offer suggestions on the broader social questions involved. He contends that "researchers need to take a stand, basing their judgment on the best level-headed evaluation of the evidence they can muster. Issues can always be avoided with calls for 'more study,' a recommendation which gives relief to those who fear acting on problems and often leads to deferring excellent opportunities for experimental action. The opportunities are not easily regained" (p. 288).

Beyond this, Toplin's conclusions as to what to do are, as he indicates, hardly revolutionary. Violence should not be excused as inevitable or as a necessary or traditional part of American culture. Aggression should not be romanticized or linked with heroism or machismo. Contempt for human life, a staple of television fare, needs to be replaced with an emphasis on the uniqueness and distinctiveness of the individual. And firearms need to be restricted. His message is summed up in two lines: "The task now is for Americans to refuse to excuse violence and accommodate to it. They must stop trying to live with the problem and start wrestling with it" (p. 292).

The prescription seems reasonable enough. Implicitly it would appear that the author is calling for a social science community more aware of its broader societal commitments and willing to do something about them.

WILLIAM CROTTY

Northwestern University

Boston: The Great Depression and the New Deal. By Charles H. Trout. (New York: Oxford University Press, 1977. Pp. xx + 401. $15.95.)

In recent years, the city of Boston has been the subject of a number of detailed historical studies of considerable importance. Works such as Sam Bass Warner, Jr.'s *Streetcar Suburbs,* Stephen Thernstrom's *Poverty and Progress,* Peter G. Knight's *The Plain People of Boston* have broken new ground in urban history as well as providing valuable insights into a single American city.

Despite some historians' conjectures about individual cities as "microcosms" or as "prototypes," only a full-fledged theory of urban economic and political development and decay will allow discrete comparisons of the complex events and patterns discussed in the urban histories noted above. This requirement, however, does not diminish the importance of single-city studies. It is entirely likely that a future comprehensive theory of urban development will owe as much to such detailed analyses as to attempts at correlating gross characteristics of large numbers of urban units.

Charles H. Trout's *Boston: The Great Depression and the New Deal* is not a pioneering work on the order of Warner or Thernstrom, but does provide an insightful record of one of Boston's crucial transition periods. Political scientists will be particularly interested in Trout's focus on the problems of the integration of public policy among federal, state and local governmental authorities. The tradition of volunteerism and a fiscally stringent "Christian charity" clashed in Boston with New Deal policies seeking to expand welfare services and welfare rights. The seraglio politics of Massachusetts led F.D.R.'s people to keep the state at arm's length. Party leaders maligned each other, to the consternation of the newly powerful federal Democrats. This situation was epitomized when James Michael Curley, defeated as an F.D.R. delegate to the 1932 Democratic convention, substituted for an ailing member of the Puerto Rican delegation. Styling himself Alcalde Jaime Miguel Curleo, Curley infuriated the regular Massachusetts Democrats, committed to Al Smith, seconded John Nance Garner's nomination for vice-president, and reinforced his image as a political clown by responding to roll calls in a Spanish accent. Curley was passed over for an important New Deal post and his rebuff was symbolic of much of F.D.R.'s treatment of Bostonians.

Trout carefully dissects the ways in which Boston struggled to adapt to the New Deal (and vice versa). Boston entered the Depression fiscally sound and with a cumbersome but comparatively effective public relief system. Ironically, this placed the city low on the list of prime targets for New Deal aid, despite the enormous support given the Roosevelt administration by Boston voters. The long-standing enmity between the State House and City Hall also led to Boston's being deprived of its fair share of New Deal relief dollars. Within the city, the byzantine patronage system often channeled funds away from where they were most desperately needed. The conservatism of capitalist entrepreneurs matched all too closely the conservatism of the Roman Catholic leadership, of ethnic politicians and upper-class governmental reformers. Distrust of New Deal politics, whether due to economic mendacity, religious antagonism or political parochialism, made for problematic relations with the new federal leadership.

To many, the foregoing will have a sadly familiar ring. Those who watched the difficulties of the War on Poverty, of Model Cities, of revenue sharing will find important—and disquieting—continuities in Trout's narrative. The book is also useful for its analysis of the decline of an urban machine in a period of federal welfare activism. Trout shows that the issues in Boston were far more complex and subtle than the standard functionalist account of the demise of the machine.

This is an important book. Trout makes extensive use of the papers of Curley; of Henry Lee Shattuck, spokesman for the political Boston Brahmins; of the redoubtable Eva Whiting White, director of the Elizabeth Peabody Settlement House and others. The documentation is meticulous and the footnotes informative. The book is written in a breezy style which produces its only disappointing feature:

the author's weakness for hackneyed expression. Thus: "the abandoned hulks of once noble ships gave the harbor a sepulchral appearance"; mobs are "howling"; disturbances are "triggered"; Boston's Cardinal O'Connell "had clawed his way to the top of the Catholic hierarchy"; "Jobs in Boston had become a political football." Lapses such as these mar what is otherwise a significant work of history and a valuable contribution to urban studies.

HARVEY BOULAY

Boston University

Decision to Prosecute: Organization and Public Policy in the Antitrust Division. By Suzanne Weaver. (Cambridge, Mass.: MIT Press, 1977. Pp. viii + 196. $14.95.)

This study describes the decision-making process of the antitrust division of the Department of Justice in the early 1970s. Based principally on interviews with officials in the division in 1971 (with some additions in 1974), it focuses on the decision whether to prosecute apparent violations of the nation's antitrust laws. Most of the book deals with the work routines of lawyers who staff the several sections of the division, although the last chapters contain fragmentary discussions of the several supervisory tiers of the division and the interactions of the divisions with OMB personnel, White House staff, Congress members and staff, the private antitrust bar, and corporate interests.

I find it difficult to identify the intellectual tradition most appropriate for evaluating Weaver's study. Several suggest themselves. In some ways it is an organizational study, although there is almost no reference to concepts of organizational analysis. We learn a great deal about the work routines of the staff and a little about communication patterns. Weaver writes about socialization into the organization, the ways in which the staff is induced to comply with organizational norms through incentives or sanctions, the impact of the task environment, and the ways in which information flows are controlled, but all this is presented in a rather unsystematic fashion since these are not the categories used by the author but must be imposed by the reader.

Another alternative is to consider this a study of a policy process or of the content of public policy. There are indications that Weaver aspires to such a focus but her materials are quite unsatisfactory for such an analysis. Antitrust policy, after all, is the product of legislation, court action and the activity of the FTC in addition to the work of the antitrust division. Weaver, however, gives only slight attention to any of these and only mentions them when their activities intersect directly with the antitrust division. One can learn little about antitrust policy or policy making from Weaver's account.

Still another way of looking at this book is to consider it as a way of understanding how inputs to the judiciary are managed. It is partly through court action that antitrust policy is executed. Again the book leaves much to be desired since there is no sustained account of litigation flow and there is no evaluation of its impact on court decision making. Moreover, there is no discussion in this volume of the decision to settle out of court through consent decrees, nor is there a detailed examination of potential differences between cases that are filled as civil complaints and those filed as criminal complaints.

The most unsettling conclusion of Weaver's book is that there appears to be no policy direction to the work of the antitrust division. The staff wants to win cases and prefers "big" cases to little ones, but the definition of "big" is ambiguous. The staff lawyers make their decisions mostly on the basis of their own intuitions of what is important. They are not given coherent policy direction and Weaver indicates that they probably would successfully ignore such guidance. That is because the division has a very strong reputation for "professionalism" which has protected it from corruption but which has also allowed it to secede from government of which it is a part and to pursue its own course in most administrations. Indeed, most assistant attorney generals in charge of the division in recent years have been more closely associated with the antitrust professional community than with the political coalition which brought an administration to power.

I must also report that this is not an easy book to read. Weaver has a dry style. She has eliminated all mention of particular cases and most names so that the account never really comes to life. One can hardly tell that she is writing of the Watergate years and of the Department of John Mitchell, Richard Kleindeinst, Elliott Richardson, ITT, and assorted scandals. Hers is largely a static account even though she discusses changes which various assistant attorney generals have attempted to initiate over the past two decades.

The antitrust division is not like the rest of the Department of Justice as far as one can tell from Weaver's account or from James Eisenstein's recent book on U.S. attorneys (*Counsel* *for the United States*, 1978). But Weaver's account will be helpful to the specialist who wishes to have a deeper understanding of the way in which an important segment of the Justice Department has worked in recent years. It is not the ideal book but it makes a worthwhile contribution.

HERBERT JACOB

Northwestern University

Using Social Research in Public Policy Making. Edited by Carol H. Weiss. (Lexington, Mass.: Lexington Books, D.C. Heath and Company, 1977. Pp. 256. $19.00.)

This collection of 16 papers is about problems which academic social scientists face in having applied research used by public policy makers. The reason for this focus is that the papers are from a symposium on "The Research Utilization Quandary," a conference on "The Market for Policy Research," and from purposefully introspective research efforts supported by government agencies which support social research. Most of the contributors are academic social scientists.

The content fairly reflects the diversity of interest and thought about the use of research in governmental decision making. One central proposition which is variously described, argued and tested, is that under-utilization occurs because of gaps between scientists and policy makers based on differences in reward systems and professional outlooks. Another major theme is that utilization must be reconceptualized for different levels of policy making and in light of social and political elements of the policy process.

Weiss' introduction summarizes the major themes. Parochial fears about the polluting effects of government-researcher interaction are set in their proper perspective; various assertions of the traditional wisdom are elaborated, e.g., that research is good, neutral, unused or simply underused. Alternative "models" of research use are developed. Weiss is committed to an uncommon modesty. Social research, she argues, is used much in the way that any information is used in policy making, and that is not to be underestimated.

These themes are developed throughout. Richard Rose differentiates between the working and social environments of public officials and social scientists according to a thinker-doer dichotomy. He stresses the necessity of consensual agreement, but argues that in the face of intractable problems there should be no unnecessary apologies from scientists for yet unsolved social policy problems.

L. J. Sharp and Renate Mayntz call for caution, modesty, and patience on the part of the scientist, after evaluating probable value conflicts between the academic and policymaking cultures. Some of the conflicts are ascribed to structural features of the government, some to political necessity, others to the shared ignorance of each culture.

Theoretical and sociological observations are given flesh in four chapters which discuss research use in various branches of the federal government. Cohen and Weiss trace the effects of scientific dissensus in research on educational desegregation, concluding that policy and research have equal impacts on each other. This is echoed by Uliasei's description of social research in foreign policy decision making.

One use of research in program evaluation is described by McGowan. Her observations about using evaluations to assist program management document the inherent difficulties which arise when evaluation is poorly conceived and used for program justification. Dreyfus suggests that the legislative process is a poor place to direct scientific social research. He describes congressional activities and resources to illustrate the primacy of political judgment and the futility of overwhelming that judgment with "masses of factual data and evidence." Rosen argues that the chief impediment to increased use in judicial decision making is the distance between science and the law. Examples of meaningful use of social research in legal testimony demonstrate that the distance is clearly bridgeable.

Six chapters are devoted to empirical studies about researcher/policy maker interaction. Useem presents results from a survey of university-based recipients of research grants and contracts. He finds that the goals of basic and applied research are often met by applications-oriented research, and that both "worlds" are well served by federal support. He concludes that "support for social science for its own sake is the price of mobilizing academic research for government's sake."

Michael Patton and others review federal health evaluation to demonstrate that utilization is too narrowly conceived by action-oriented social scientists. They illustrate the importance of understanding particular contexts of decision making when searching for use-impact. Knorr reports results from interviews with decision makers in Austrian federal and municipal agencies. He shows use to be more common than is normally perceived and finds no support for notions that social research is merely symbolic. Basing his conclusions on a study of over 500 specific examples, Caplan lays out a set of minimal conditions found to promote utilization. These include: mutual appreciation of the scientific and nonscientific aspects of the policy issue; well-defined issues which beg for information; and reasonable, non-counterintuitive findings which suggest feasible solutions.

Robert Rich presents findings about the use of data produced to inform several agencies about public perceptions of issues. Results are found to be used for action conceptualization rather than for action. He concludes that *action* is an unnecessarily restrictive utilization criterion. In the concluding chapters, Weiss and Bucuvalas present more evidence that utilization is more frequent than a simple review of policy change would reveal, and Rein and Schon argue for the use of policy research in problem setting as well as problem solving.

All of this may sound repetitious, or even self-serving in light of the commitment to applied research which the contributors share. Yet, if we consider the certainty with which some academic social scientists assign illegitimacy to applied research, the evidence and arguments contained in this book serve valuable purposes. The desirability of a pluralism in the research enterprise is underscored, and the case is made that social science indeed has contributed to improving public policy making. Both signify an emerging maturity in the social research enterprise which deserves wider recognition.

CHARLES N. BROWNSTEIN

National Science Foundation

Striking a Balance: Environment and Natural Resources Policy in the Nixon-Ford Years. By John C. Whitaker. (Washington, D.C.: American Enterprise Institute for Public Policy Research, and Stanford, California: Hoover Institution on War, Revolution and Peace, 1976. Pp. xv + 344. $5.00, paper.)

Anyone interested in federal legislation on natural resources and the environment will find it much worthwhile to buy and read a copy of this book. The book is a detailed, factual, highly readable account of the Nixon-Ford administrations in these fields.

John Whitaker was a worker for Nixon in the unsuccessful 1960 campaign and again in the successful 1968 campaign. He then joined the administration on a full-time basis, first as part of the White House staff and in 1973 became Under Secretary of the Interior, continuing there until 1975. He was very much an administration man and his book is very much an insider's view of events during those years. The insider has great advantages because of his close involvement and his intimate knowledge of events; he also suffers severe disadvantages because of his lack of a detached view. One can often benefit greatly from an insider's account if one recognizes its characteristics.

This book contains a great deal of information. It taught me some things about public land administration in the days of my active participation in such administration, which naturally increases my respect for its detailed coverage. Whitaker did his homework diligently or he had access to persons who knew their history and their facts. The book is full of dates, facts about specific legislation, incidents and personalities, and about actions taken. There is almost exclusive reliance on government reports, with relatively little use of independent documents. But the use of government documents is extensive and, as far as an outsider can tell, complete.

Anyone interested in federal governmental processes will find particularly rewarding Whitaker's discussion of the "iron triangle" of middle-level bureaucrats, private interest groups, and concerned members of Congress, among whom exists a symbiotic relationship. Whitaker quotes David Broder (p. 45), who in turn was quoting John Gardner, so that he fully recognizes that neither the idea nor the term is original with him. But he contributes instances and describes the working of the "iron triangle." All of this is highly pertinent to the situation in 1977 when a new president is struggling with the iron triangle, not only for resources and the environment but for many other aspects of governmental activity as well.

Whitaker describes the Nixon years graphically, at length and in detail, as a period of congressional initiative to pass environmental legislation without much consideration of costs, effectiveness, and probable side effects. He shows how politics—including presidential aspirations of notable senators—led to legislation to meet the interests of the most dedicated environmentalists. He convincingly documents the fact that much of the resulting legislation was hasty, ill-considered, largely ineffective in operation, often requiring postponement or repeal because of unintended effects. One can agree with at least some of this analysis without necessarily agreeing that Nixon played as constructive a role as Whitaker attributes to him. There is virtually no reference to Watergate and its consequences (Watergate does not appear in the index). An outsider cannot help but wonder what a different president from Nixon might have accomplished in "striking a balance" between environmental protection and economic growth.

MARION CLAWSON

Resources for the Future

Political Philosophy and Rhetoric: A Study of the Origins of American Party Politics. By John Zvesper. (Cambridge, England: Cambridge University Press, 1977. Pp. vii + 237. $16.95.)

John Zvesper uses the period of the origin of American political parties, which he places in the 1790s, to present a new explanation of the role of political parties. He regards this role as being different in America from the British practice. The traditional view of British parties is the same as that presented by Richard Hofstadter in his *The Idea of a Party System.* In addition to rejecting Hofstadter's concept, Zvesper likewise rejects the group concept of politics as presented by Noble Cunningham on the Jeffersonians; or the writings by William Goodman on the two-party system; or my own work on *The Adams Federalists;* or the earlier work by Charles A. Beard; or that by V. O. Key.

What, then, is the interpretation presented? Zvesper believes that America has a system which actually is a one-party operation. He declares (p. 6), "The American two-party system is truly a capacious one-party system." There is sufficient consensus around general principles that there is general agreement over fundamentals. Thus he postulates that there is alternation of individuals to carry on the functions of government, but there is reasonable agreement over basic concepts.

The basic concepts on which he finds agreement are those originally adumbrated by the Federalist writers of the 1780s and 1790s. Zvesper agrees with other writers that the framers of the American Constitution did not anticipate competing parties, yet they founded the first parties. The Federalist concept of government, as stated by such writers as Madison, Hamilton, Fisher Ames and John Adams, is based on the type of argument developed by Thomas Hobbes: "That government and civil society are necessary correctives to the war-like state of nature. Man was not naturally political but government was made necessary by his self-interested nature." Then certain individuals in their self-interest serve in government. In Hamilton's terms, "The love of fame [is] the ruling passion of the noblest minds" (p. 14). Thus Zvesper has an ideological interpretation of politics which transcends the ordinary concept of the interplay of economic and social interest groups. The Republicans, on the other hand, "argued that the state of nature was not so terrible. . . . But men are naturally social. The natural harmony of self-interest together with the operation of a natural moral sense made government less urgent than the Federalists imagined" (p. 14).

From this Zvesper concludes that parties are more based on this concept of human nature than they are on clash of interests. The Federalists' analysis, as expressed in the Federalist rhetoric, has proved more realistic than the Republican. Therefore, the Federalist concept was eventually adapted by America and is the best explanation for the American party system.

I would venture several criticisms of the above analysis:

(1) Madison, after all, for most of his career was a Republican and not a Federalist. His definition of parties is therefore more common to both the Federalists and the Republicans. I feel that Zvesper has not given as broad a definition of Republican principles (as is presented, for example, by Adrienne Koch in her book on the philosophy of Thomas Jefferson) or that Madison belonged solely to the Federalists, even though Zvesper does put him with the Republicans in the last part of his book.

(2) Zvesper's work never clearly defines what is meant by "rhetoric."

(3) Many of the Federalists such as John Adams believed along with Madison that a balancing of interests was important. I feel that in this sense more than ideology is involved in what parties represent.

(4) Finally, I agree that there is a consensus around certain "rules of the game" like periodic elections, opposition parties, the rule of law, a bill of rights, but I think other writers have shown that there are meaningful economic and social issues and even other policy issues between the parties, and it is an exaggeration to term the American party system as basically only over who runs the government and not over policies and principles.

MANNING J. DAUER

University of Florida

Comparative Politics

Chinese Politics and the Cultural Revolution: Dynamics of Policy Processes. By Byung-joon Ahn. (Seattle and London: University of Washington Press, 1976. Pp. xi + 392. $15.00.)

By now numerous books have been published on the elite conflict and purges on mainland China from 1966 to 1969, known as the Great Proletarian Cultural Revolution. Since information on that subject comes mainly from two sources: denunciations in the official mass media of the victims of the purge and exposes (against the victims also) in the papers published by various Red Guard or rebel groups, most works on the Cultural Revolution are highly uniform in their descriptions and interpretations. Almost all of them deal with the conflict between the so-called "radicals" headed by Mao and the "moderates" (or "pragmatists") led by Lin Shao-ch'i and Teng Hsiao-ping. This latest volume on the Cultural Revolution by Byung-joon Ahn does not break that uniformity.

Though Ahn states at the outset that he would focus his study on the events leading to the Cultural Revolution, particularly those of 1958-1966, his narration actually starts from the land reform in 1949 and ends at the death of Mao in 1976. The book thus covers every major known political event, domestic or foreign, since the birth of the People's Republic. He goes over the same ground and reiterates the same generalizations already made by numerous scholars concerning the land reform, the agricultural collectivization, the Great Leap Forward and the Cultural Revolution on Mainland China. Since Ahn has overextended himself in his narration, his later chapters, on the Cultural Revolution itself and the developments after that, read more like a chronicle of events with little systematic or penetrative analysis. On the whole, Ahn's study duplicates much of *Power and Policy in China* by Parris Chang (University Park, Pa., 1975) and *Mao's Way* by Edward Rice (Berkeley, 1972 and 1974).

Throughout the book and particularly in the "Epilogue," Ahn describes Mao as the prime mover in China's policy process. Yet nowhere in the book does Ahn discuss Mao's views toward social and economic problems in a systematic and comprehensive way. For example Ahn often uses Mao's preference for "mass line," "mass mobilization," "socialist transformation" to explain Mao's conflict with others. But Ahn has not clarified these concepts in any rigorous or exact way, only mentioning them here and there in a sketchy fashion, repeating well-known generalizations about Mao. Further, how Mao really acted in the policy process which Ahn defines as consensus building is glossed over. Thus we are told that "Mao somehow made the Party accept" (p. 16) his "rural model of socialism," that "Mao eventually succeeded in having" (p. 17) the Party agree to his radical scheme of collectivization and that Mao "skillfully maneuvered" (p. 20) the Hundred Flowers campaign in 1957 into an "anti-rightist" movement. Even if we grant that Ahn is more interested in the events of 1958-1966, it is still important for him to show the consistency of Mao's "operational code" over various policies. Ahn's narration of Mao's conduct in the Cultural Revolution sheds no new light on the matter.

Though most chapters repeat well-known facts and interpretations, they are at least based on the careful and judicious works of other scholars. In chapter 6—"The Army's Emulation Campaigns"—Ahn suddenly gives vent to his own, mostly unsubstantiated, generalizations. He does not give evidence to support the statements that the leadership and the rank and file in the army were more responsive to Mao's mass campaigns than the Party (p. 123), that the only dissenter to Mao's ideas at the army's top machinery was Lo Jui-ch'ing (p. 124), that the "four good" company campaign was effective in the army (effective in what sense?) (p. 126), that the army's guerrilla tradition was still regarded as a virtue in the army (p. 125), that the General Political Department made every effort to ensure that all military units would implement Mao's policy (p. 135) or that soldiers were more ideologically motivated than any other sector of the population (p. 135) (more than the students?!).

Regarding issues such as the ideological motivation of soldiers in China and the popularity of the guerrilla tradition in the army, other sources or scholarly works refute Ahn's generalizations (for the former, see *Kung-tso t'ung-hsun;* for the latter, Whitson, *The Chinese High Command*). On page 134, Ahn inexplicably states that the army proved to be more amenable to Mao's method of mass movement "as shown by the various campaigns within the army." What is shown is simply that the official mass media "reported" those mass campaigns; one would think that Ahn would not readily equate reportage with actual practice. Furthermore, in most instances the conduct of the army in the Cultural Revolution should have informed Ahn of the falsity of his generalization. Had he analyzed more in depth the differences between the model of Lei Feng and that of Wang Chieh (he regarded the two as similar models), Ahn would have discovered another clue to the ineffectiveness of the "Learn from Lei Feng" campaign in the army. Ahn's statement that the campaigns in the army from 1964 on were a training exercise for the forthcoming Cultural Revolution is also erroneous (p. 135). The campaigns in the army were carried out by a central command. There were no Red Guards, no rebellion against superiors and no "grand alliance" among rebels. The army, as shown in their conduct in the Cultural Revolution, was as unprepared as the cadres for the "mass politics" of the Cultural Revolution.

For students of Chinese politics, Ahn's book adds nothing to what they already know. For non-China specialists, however, it presents a clear summary of major public policies and disputes on mainland China from 1949 to 1976. But even the non-specialists must discount many of Ahn's generalizations.

ALAN P. L. LIU

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Power and Ritual in the Israel Labor Party: A Study in Political Anthropology. By M. J. Aronoff. (Assen, The Netherlands: Roy Van Gorcum, Ltd., 1977. Pp. xiv + 184. Dfl. 39.00, paper.)

This is a competent set of footnotes to the conventional wisdom about the Israel Labor Party. That party (in its various incarnations) created, then thrived in, a political atmosphere marked by corporatism, exclusiveness and coercion. In writing this before Labor's fall from grace in May, 1977, Aronoff was careful to touch all bases by suggesting that this atmosphere could produce an electoral defeat—a bold conjecture, if Labor had been replaced by a different kind of party. But it was not.

Aronoff writes as a political anthropologist, and an unabashed courtier to the late Max Gluckman. He claims to bring a new approach to the study of political parties, that of participant observation informed by anthropological categories. This, Aronoff suggests, will lead to new insights, and to fresh perspectives about the nature of political party operations. But it does not.

The major category employed by Aronoff is "ritual." Ritual, as we know, has the primary function of mollifying the masses and appeasing the gods through symbolic action or speech. Most public or quasi-public political gatherings are, by nature, ritualistic; and Aronoff gathered his information almost entirely from such party gatherings. Therefore, he found ritual where it should have been, and where we have always recognized its manifestations. In terms of his major category, as it is applied to the structure and function of the Israel Labor Party, Aronoff does not take us beyond the conclusions of Peter Medding's *Mapai in Israel,* published some years ago.

As it happened, however, Aronoff observed the Labor Party during *the* critical moments of its long history. Through this stroke of good fortune, we can justify the book by its superb compendium of rare anecdotal material.

It is in his failure to grasp for larger definitions that Aronoff's work brings disappointment. Medding, for example, was able to work with the realization that Labor has been not only the manager of the state of Israel, but was, in most important respects, the manifestation of the state itself. In simple formulation, Labor is the creature of the dialectic that shapes modern Jewish history; it is the locus for the unfolding of contradictions that, by all appearances, have no ultimate point of resolution. In the largest sense, Israel is itself one of those contradictions.

That Aronoff failed to grasp this fundamental view is perhaps not so much his personal failing, as it is the failing of the brand of cultural anthropology into which he was trained. In the weak dilution of this book, Aronoff presents Labor, vaguely, as a microcosm of the larger Israeli culture.

In presenting the party as a surrogate for the culture, however, the writer should assume the responsibility for demonstrating the viability of the substitution in all major categories. This is too much to ask of a single book. It is certainly too much to ask of research effectively limited to observation of secondary levels of party activity.

But, to do justice to the reader, Aronoff should have been more thorough at those points where the party's activities touched on vital themes of the larger culture. Aronoff's one venture into this overlap was ineffective: in great detail, he outlines the party's internal crisis over the nature of religion/state relations. But the failure of the party to resolve this issue is not, as Aronoff would have us believe, a failure of the party to resolve the demands of internal clients. It is, rather, a far broader failure of the polity, *cum* culture, to resolve a fundamental contradiction between the national religious heritage and the slow corrosion of secularization.

Aronoff's failure to come to grips with the more metaphysical qualities of his subject does not limit the main content of his book, so much as it limits the general conclusions he wishes to draw about the nature of political parties, or about this moment in Labor history.

Aronoff's forte is the reporting of dramatic moments in the recent intra-party struggles—particularly after the perceived debacle of the October, 1973 war. Those passages are rich in detail, and fine in their sense of the moment. They are the all too rare enrichments that conventional studies of politics miss, and become dehumanized by missing.

On reading these passages, I regret that Aronoff's access was limited to second- and third-level party activities, and that his personal informants were, for the most part, limited to those outside the main action. However, the fact of these limitations itself speaks to conventional wisdom about Labor politics in Israel: very few are ever called into the most sacred forums.

Aronoff is to be complimented for the energy and richness of detail in this work, and for the questions that he implicitly reminds us are still to be answered.

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Modern German Nationalism. By Abraham Ashkenasi. (Cambridge, Mass.: Schenkman Publishing Co., Halsted Press, 1976. Pp. xv + 222. $12.50.)

Few will dispute the claim that nationalism in Germany has served neither that country nor the rest of the world particularly well. A phenomenon that elsewhere was bothersome or perhaps naughty became a force driving the German nation to catastrophe. Myriad studies have related the ideas and actions of National Socialism to a long-term historical development of authoritarianism and nationalism. The taint given nationalism by that era has been clear enough in post-war German politics. Main-line political parties have sought to avoid too obvious an association with it.

Nationalism nevertheless remains at least a residual force in Germany. Not only are there still a few extremists around but also the general citizenry maintains varying levels of nationalist sentiment as well. Given the past, questions concerning the nature of this nationalism and the sources of its support are both interesting and important. Is modern German nationalism simply a new version of traditional authoritarian nationalism? If not, what is it? Is it likely to gain widespread support through political parties like the Christian Social Union, perhaps as this organization expands beyond Bavaria to the rest of the Federal Republic? Is it likely to gain powerful support in the Bundeswehr?

The chapter headings of this book lead one to expect answers to these questions. Topics include the historical and sociological context of German nationalism, postwar political attitudes, the organization of nationalists and their struggle for power in postwar Germany, the Bundeswehr, the Christian Social Union, and a content analysis of three right-wing newspapers.

Each chapter cites and uncritically reports at some length some of the relevant earlier work on these subjects. Among the conclusions reached are that the tarnished image of nationalism will have to be repaired if it is to have the influence in German society that it had in the past; that keeping the CDU/CSU to the political right is perhaps more important for organized nationalism than is electoral strength for the small neo-fascist organizations; that the officers' corps of the Bundeswehr could be a major galvanizing element for German nationalism; that modern nationalism as represented by Franz Josef Strauss has a great deal in common with the program of pragmatic fascists who wish to preserve capitalist structures and their managerial and political elites; and that there exists a hard core of 10 to 15 percent of the population that is extremely nationalistic and could be counted upon to lead the rest if organized nationalism and other conservative elements were effective in influencing susceptible social groups.

It is somewhat difficult to identify the central thesis or focus of the book. If, for example, there is evidence that authoritarian attitudes continue to exist not only in the army but also among a minority of the general public or that right-wing partisans are not altogether uncomfortable in Strauss' organization, does this or does this not indicate a revival of catastrophic nationalism? If neither, what does it signify? It is also difficult to understand precisely what is meant by modern German nationalism. The author cites a number of ideas and definitions from the literature but no single concept emerges from the plethora of detail. Often the term seems to be a synomym for opposition to social change and the authoritarian conception of political control. This is unfortunate, both because it ignores national emotions and perceptions that are not right-wing and because whatever is perceived to be right-wing comes to be nationalism. The book tends to be a polemic without a fully specified target. Answers to the important questions on Germany's future must be found elsewhere.

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The History of Parliamentary Behavior. Edited by William O. Aydelotte. (Princeton: Princeton University Press, 1977. Pp. xiv + 321. $22.50, cloth; $9.95, paper.)

This book is not a history of parliamentary behavior but rather a collection of essays dealing with quantitative approaches to the study of legislatures. The thread tying these essays together is that individually and collectively they demonstrate the utility of quantitative techniques for the study of legislative history. The articles are classified in three categories: (1) recruitment to legislatures, (2) quantitative descriptions of legislative behavior and (3) explanations of legislative behavior. Within these three broad categories there are a number of different types of analysis. In two of the articles a time-series approach comparing the institution to itself at varying times is used to show changing patterns in recruitment to legislatures. Some of the studies focus on European legislatures, thus offering cross-national comparative analyses, while at lest three of the articles use quantitative techniques to explain or describe behavior during a critical phase in the legislature under consideration. Given this organizational scheme, the book can be evaluated both as a vehicle to demonstrate the utility of quantitative-historical approaches to legislatures and the articles can also be judged individually on their substantive and methodological contributions.

Aydelotte's selection of articles varies widely in terms of both focus and technique. The variety of time, place, approach and technique well serves the purpose of demonstrating the utility of quantification. I suspect that demonstrating this point is of far less importance to political scientists than to historians. In general, political scientists who study legislatures are already convinced of the importance of quantitative approaches, although a number of the articles, such as the Clausen and Holmberg piece, put familiar techniques to innovative uses. In short, if anyone had doubts about the utility of quantification in studying a diversity of legislatures and problems, this book should erase such doubts. Political scientists especially, however, should learn about the uses of history to test developmental generalizations about legislatures. Students of American legislatures are particularly insensitive to the changing nature of legislative systems. Pederson's article on personal circulation in the Danish Folketing is particularly instructive in this regard. His use of a time-series design sheds light on a number of important theoretical problems concerning both institutionalization and recruitment. In short, it is a model study.

The more relevant aspect of the book for political scientists is the substantive importance of the various articles. A number of these articles merit the serious attention of legislative scholars. The two articles in the recruitment section are primary examples of such work. Douglas Price's study of congressional careers and committees in the nineteenth-century Congress should be required reading for students of the American Congress. In his usual fashion Price asks interesting questions and provides appropriate measures to answer said questions. Pederson's article has already been mentioned as extremely worthwhile.

Of those articles dealing with descriptions and explanations of legislative behavior I found both the article on the making of the Mexican Constitution (Peter Smith) and the Hosking and King article on the Liberal party in the 1906-1914 period to be informative. Smith's study of constitution making shows that social background variables can be important, and that factor analysis, innovatively used, can tell us something important. Smith's use of both historical sources and quantitative techniques is impressive.

The study of voting in the Swedish legislature by Aage Clausen and Soren Holmberg shows that with the proper methods a policy dimension can be applied in a disciplined multiparty system. The result is an article that both tells us something about the Swedish legislature and shows us how to measure policy in a partisan context. The only article in the collection that failed either to demonstrate the utility of quantitative approaches or to be substantively informative was the Prost and Rosenzveig study of the French Chamber of Deputies in 1882-1884. Although this was an important period in French history, the factor-analytic techniques seem to rule the authors to the extent that neither the technique nor the findings is clear. The articles that have not been specifically mentioned in this review range from a study of the Norwegian Storting in the 1960s to the British House of Commons in 1841–1847, and their overall quality is good.

In sum, the individual articles are good and collectively they clearly demonstrate the utility of quantitative techniques in studying the history and development of legislatures. Moreover, a good number of the articles should be read by students of legislative behavior because they contribute to either our substantive and methodological knowledge, or both. I don't think one can ask more of a collection of essays. Aydelotte has both shown the utility of quantification in the study of legislative history, and selected a good collection of individual essays. Thus in my judgment the book is a success.

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Representation in Italy: Institutionalized Tradition and Electoral Choice. By Samuel H. Barnes. (Chicago: University of Chicago Press, 1977. Pp. xi + 187. $16.00.)

Samuel Barnes' study of Italian political attitudes is a product of the University of Michigan's series of comparative electoral surveys started in the 1960s. It is competently executed, presented in spare and precise prose, and it deals even-handedly with a political system that has seldom been so systematically studied from the point of view of political attitudes and behavior. Summarizing and adding to Barnes' published articles on the Italian electorate, the book fills a large gap in our knowledge.

But it is not the gap announced in the introduction—e.g., "the process of representation at the time of the 1968 legislative elections" (p. 1)! The 1968 elections were certainly the setting for the study, and Barnes shows once again, as he did in 1967, with his *Party Democracy,* that he handles survey materials with finesse and without mystification. Moreover, the study deals with both elite and mass-level survey materials, including in the former both a dense parliamentary sample and a sparser, but still adequate, sample of local government elites. Thus, both the governed and the governing elements in the "process of representation" are present in the study.

It is the linkage between them that remains incomplete. Representative linkages are studied sociologically, in policy terms, and in terms of the perceived contacts between voters and their representatives. Barnes shows that sociological parallels are of little importance, that "there are no strong relationships between representatives' opinions and those of their party's identifiers living in the constituencies" (p. 121), and that reported personal contacts between legislators and voters appear to be extensive. What the study does not do is to help us to understand the relationship between constituency factors and the behavior of representatives.

Barnes rightly rejects the study of roll-call votes in Parliament as a measure of the impact of constituency views on legislators—in Italy's disciplined mass-party system, this would make little sense as a research strategy. He concentrates instead on the parallels in the issue orientations of voters and representatives, coming up with a number of interesting findings in the process, but leaving largely unanswered "the impact that variations in the views of the represented have on the views of the representatives" (p. 117). In other words, this is a parallel study of the attitudes and orientations of voters and political elites, but it is not a study of the "process of representation" in Italy.

Once we turn to the book on the ground of its own data base, Barnes' contribution is substantial. First, he goes well beyond the classical ISR survey design by including data on three levels of the political system, including a good deal of descriptive material on the Italian party system. Second, he underscores the importance of social and religious networks in providing cues for mass voting behavior, replicating and refining the findings on the importance of religious belief found in earlier surveys. Third, Barnes focuses explicitly on the important role of party in organizing nominations, electoral campaigns, parliamentary behavior and even, it would appear, the mental universe of many voters in Italy. The average Italian voter is not a militant, but sees the political universe in party-colored glasses.

This emphasis on party is not accidental. It is a logical follow-up to Barnes' earlier work on socialist militants, emerging clearly in Italy's history and political traditions; it was a central part of the research design that Barnes presents in an appendix to the study. Though the mass public is less supportive of party than are the elites, "both place the party ahead of the individual deputy," writes Barnes, in their attitudes towards various criteria that should guide the legislative voting of the latter (p. 133).

Because party is so important, electoral change has been quite limited in Italy. Through it, the South was electorally integrated within the national political system. And by the intermediation of ideological party machines and their conflicts, Italian electoral politics remains highly ideological, even though the policy game "is as pragmatic and personalistic as the electoral game is ideological and party related" (p. 159).

Does the importance of party in the Italian electorate inevitably lead to ideological politics? Though Barnes argues that a spatial model "fits amazingly well with attitudes toward major issues" (p. 97), his data also show that a majority of the Italian electorate place themselves in the middle of the ideological spectrum. Indeed, the largest group of voters, the Christian Democrats, virtually mirror the left-right self-placement of the electorate as a whole. "The Italian system," Barnes concludes, "does not appear to be highly polarized in left-right terms" (p. 104). His data thus question the facile assumption that there is, in Italy, a substantial reservoir of extremist voters drawing the political parties towards the extremes and away from the center of the political system.

Ideology, of course, is more apparent among the elites that Barnes studied than among the mass public. The sources of the elite's ideological thinking appear to lie in the socialization of the greater part of it under the hothouse conditions of war, fascism and resistance. By the same token, ideological politics is less obvious in the attitudes of more recently recruited leaders. Barnes writes, "there has been an overall decline in the percentage listing ideological reasons [for joining a party]" (p. 139). These differences raise serious questions about the "fragmented-ideological" model of Italian politics that many specialists have inherited from the immediate postwar period, and suggest, in addition, that among younger, more pragmatic, party leaders, a good deal of political exchange may be going on under the surface.

In summary, this is a useful study of attitudes and orientations of Italian voters, and of local and parliamentary elites in the late 1960s that verifies the importance of party in that system and provides much evidence that qualified and refines our inherited wisdom about it.

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Up to the Mountains and Down to the Villages: The Transfer of Youth From Urban to Rural China. By Thomas P. Bernstein. (New Haven: Yale University Press, 1977. Pp. xiii + 371. $17.50.)

Up to the Mountains and Down to the Villages is an outstanding study of the Chinese policy of sending educated urban youth down to the countryside to participate in agricultural labor upon completion of their secondary education. This book is more than just a thorough study of an interesting, and in many ways unique, policy of the Chinese government. In fact, it can serve as a model for future book-length studies about China. This book is particularly useful because the author has avoided the temptation to write a whole system analysis of the Chinese political process. Through his focus upon a particular policy area, we gain a deeper understanding of that policy and the lives of the individuals affected by it. This type of focus allows us to gain some insight into the impact of regime policies on society and gives us some empirical indicators of the meaning of the Chinese revolution.

Bernstein argues that the policy of sending urban youths "up to the mountains and down to the villages" is a response to three specific problems: it is designed to eliminate real and potential urban unemployment; to reform the values of urban school graduates who are unable to secure a place for themselves in a higher-level school or a job in the urban sector; and to transfer educated personnel to the countryside to contribute to rural development (p. 33). He discusses the process of mobilizing youths to go to the countryside, the difficulties and problems of their adjustment and the contributions they make to rural development. Each topic is discussed at some length and an impressive amount of detail is offered. Bernstein also makes valiant attempts to provide statistical profiles of who is recruited, where they are sent, what labor they perform and how the press covers the rustication program. What emerges is a remarkably clear picture of the policy and its many implications.

Bernstein discusses a number of problem areas in the transfer of urban youth program. Perhaps the most pressing problem is that transfer to rural areas violates the aspirations of most urban educated youths who would prefer to remain in school or find work in the urban sector, in spite of regime arguments about uniting with the peasants to develop the rural sector. The sense of deprivation of these transferred youths is made more acute by the indefinite lengths of their stays in the countryside and the regime demands that they become integrated with the peasants. The policy of the indefinite sojourn, while ideologically understandable, seems to undermine the ability of those youths to adjust to rural life and to contribute to rural development. A policy of guaranteed permanent transfer might force youths to accommodate themselves to their misfortune. Even better, a policy of rotating urban youths to the countryside for fixed stays would equalize the burden of rural transfer and still assure urban youths that they would not be permanently consigned to the more arduous rural existence. Bernstein argues that city youths would have a less serious morale problem and be more disposed to make a contribution if they knew that their stay in the countryside was unavoidable but limited. Of course, this would constitute a major ideological concession by the party leadership, namely, that the integration of urban intellectuals with the peasant masses was neither desirable nor feasible.

A second question of considerable interest concerns the contribution towards rural development that the urban youths are permitted to make. Most are assigned the roles of agricultural laborers, where because of skill, strength and motivation they are only marginally productive. These youths are not contributing to the "goal of eliminating the gap between mental and manual labor. These urban youths have ended up on the labor side of the gap; they have not bridged it" (p. 241). Bernstein cannot avoid the conclusion that the city youths have not lived up to their potential usefulness as catalytic agents to help bring about rural modernization. To change this situation, the morale or urban youths will have to be improved; the curriculum of urban schools will have to be reformed to insure that graduates are able to contribute to rural development; the resources will have to be made available so that urban youths can be sent to the poorest regions where their services are most needed; and high-level supervision will have to be provided to insure that urban youth are treated properly by the host peasants and assigned useful work.

Bernstein believes that the Chinese government has succeeded in controlling urban unemployment through the transfer program but has achieved substantially less success in transforming the attitudes of the "sent-down" youths and has not permitted these youths to make much of a contribution to rural development. His evaluation is critical but fair and well supported. He believes that the goals of the program will be modified as the regime grows more mellow and that less ideologically demanding aspirations will have a greater chance for success. Given the appearances of recent developments in China, this seems to be a very reasonable prediction. However, even if Bernstein's predictions fall short, this is a very fine book that should remain useful for many years.

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The Study of the Middle East: Research and Scholarship in the Humanities and Social Sciences. Edited by Leonard Binder. (New York: John Wiley, 1976. Pp. vii + 648. $25.00.)

Middle East studies are alive and reasonably well, reports a committee of the Middle East Studies Association of North America in this useful volume. Ten specialists describe and assess their respective disciplines, providing a good sense of strengths and weaknesses along with extensive bibliographies. Leonard Binder contributes an overview chapter, in which he tries to sort out the practical problems and epistemological conundrums affecting area studies in general and Middle East studies in particular. The main practical problems include funding, language skills, ideological distortion and "a great deal of incompetent scholarship" (p. 16). The epistemological difficulties involve orientalists vs. area studies vs. discipline approaches. There is also a nagging uncertainty, insufficiently discussed here, that the very concept "Middle East" lacks analytic validity. Nevertheless, according to Binder, Middle Eastern studies have made progress in two ways: Middle East specialists are now found in many academic departments, where they are accepted if not respected; they have also grown in epistemological sophistication. Orientalism and naive area-studies approaches have given way to discipline-oriented analysis which, at its best, also takes account of the area's cultural context. The Middle East specialist of the future, says Binder, will (or should) have mastered more than one discipline, and will have better competence in Middle Eastern languages, philosophy and history than his predecessors. *Inshallah.*

William Zartman has provided a comprehensive assessment of political science on the Middle East—workmanlike and critical. It will interest not just Middle East political scientists but also those in comparative politics, modernization, and international relations. He cites as seminal works of the post-World II period Rustow's chapter on the Middle East in Almond and Coleman's *Politics of the Developing Areas,* Lerner's *Passing of Traditional Society* and Halpern's *Politics of Social Change in the Middle East and North Africa.* The other 311 writers who appear in his bibliography have advanced the "art" of Middle East political science most successfully through country studies, although these studies rarely test theory and focus too narrowly on a specific structure. Many political scientists have taken a modified institutional approach, bowing to the exigencies of the research climate which usually precludes survey and other quantitative techniques, and have focused on a particular relevant structure, such as parties and elections, and especially the military. Zartman feels that Middle East political scientists have tended toward the descriptive rather than the analytical. But there has been improvement, notably the adoption of structural-functional analysis and a greater concern for empirical rigor. On balance, he believes that Middle East political scientists have made important contributions to the discipline in the study of parties, elites, the military, and political reactions to social change.

But there are serious problems too. Ethnic politics have been neglected and "class analysis is scarce." Replication of studies is lacking; teaching beyond the elementary level is rare; and field research opportunities remain quite limited. The study of Islamic law and political philosophy has unaccountably lagged. International relations is perhaps the weakest sub-field, uninformed by new theory, distorted by political pressures (the Arab-Israeli conflict casts its ugly shadow here), and frequently unable to lift itself above the current events level. The few quantitative efforts in international relations have yielded more methodological controversy than interesting findings. Among Zartman's priorities for the field are the following: studies of *homopoliticus mediorientalis*—attitudes and values; local politics; elite circulation; revolution; cross-national institutional studies; policy processes and "outcomes." Collaborative research is needed to remedy the weaknesses in all these areas.

Even those who disagree with some of Zartman's points or find some gaps in his survey will find it enlightening. And political scientists of the Middle East will find the assessments of sister disciplines in this book equally valuable—notably Albert Hourani on history, George Sabbagh on sociology, and especially Richard Antoun on anthropology. Nor should Charles J. Adams' essay on Islamic religious tradition be neglected, in particular his stress on the importance of language training and his politely critical discussion of social science approaches to Islam.

In terms of productivity, Middle East political studies seem strong. Yet one senses a persistent deficiency both in sophistication of theory and method and in contextual validity. This backwardness may be due in part to the tenuous relationship between the Middle East politics "community" and the political science profession. In the last four years (1972–76) the *APSR* published only three articles dealing with Middle East politics; *Comparative Politics* only five; *World Politics* only six; *International Studies Quarterly* only four. The bulk of political analysis appears in area-oriented journals or magazines where conventional political science is overshadowed by historical, philological, ideological, or current-events preoccupations. Without asking the reasons for it, one can only observe that the degree of separation between area and discipline is regrettable.

Another problem is the gap—physical, linguistic, political—between Western Middle East studies scholars (not just political scientists) and their area of study. Fortunately, this gap is narrowing because Middle Eastern governments and universities are rapidly developing their own research capabilities and their own competent scholars (notwithstanding Binder's astonishing statement on p. 3 that the wealthiest countries of the area "least value the Western ideal of scholarship"). One of the best ways to deal with Zartman's priorities is to pay far more attention to joint research ventures with our Middle Eastern political science colleagues.

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Communism in Italy and France. Edited by Donald L. M. Blackmer and Sidney Tarrow. (Princeton, N.J.: Princeton University Press, 1975. Pp. xviii + 651. $25.00, cloth; $9.75, paper.)

We are often given the choice between single-country studies which make no effort to go beyond the loving analysis of detail, and multination studies that try to abstract common features or create grant typologies with little regard for the contextual significance of characteristics and criteria of classification. This volume of original essays on the Communist parties of France and Italy demonstrates that a much more fruitful approach is available. Although most of the essays pertain only to one country, their parallel treatment of party activists, grass-roots organization, and alliance strategies goes far toward distinguishing what is unique to each milieu and what is a common feature of the predicament of Western European Communist parties.

For example, both Denis Lacorne and Peter Lange establish the predilection of the parties in France and Italy for nonminimal coalitions in forming interparty alliances. In a beautifully matched pair of articles on PCF and PCI relations with trade unions, George Ross and Peter Weitz illustrate the common dynamic of growing autonomy of Communist-linked union confederations, but also convey how and why the relationships differ. Several careful analyses of ideological developments also soften the dichotomy of the Italian party as ideologically loose and the French party as rigid. Without denying the relatively greater orthodoxy of the PCI, many of these studies confirm that both parties face a continual struggle to reconcile Marxism with populist reformism, tactics with principles, the short term with the long term. Tarrow's essays on public officeholders and his concluding overview contrasting the parties are explicitly comparative, reflecting great sensitivity in integrating historical and cultural considerations to explain their divergent strategies. Thus the volume as a whole is a marvelous demonstration that comparative studies can preserve contextuality.

The focus of most essays in this collection is the organizational and social fabric of each party. Is the painstaking research of organizational structure and day-to-day interaction really necessary for explaining the significant outcomes of party strategy and issue positions? It would certainly be *easier* to isolate the analysis of strategy formulation from the laborious examination of interactions among party leaders, activists and electorates; many studies (particularly those appearing in the foreign policy journals and the popular press) treat the parties as pure, monolithic strategists. Yet the studies in the Blackmer-Tarrow volume not only emphasize party structure, they also *demonstrate* that the structure must be taken into account to understand the parties' strategic choices and the constraints on these choices. For example, Blackmer, in a broad treatment of Italian Communism, argues that the party's organizational emphasis on mass structures has focused the PCI leadership's attention on a broad range of populist issues. Tarrow makes a convincing argument that the PCF is even more constrained by its structure, which because of its hierarchical organization is inflexible on issue positions and narrow in the range of issues it addresses. George Lavau provides a fascinating organizational explanation of how the PCF rank and file may constrain the party leadership from a future decision to overturn the liberal-democratic "rules of the game"; many PCF members and sympathizers accept the political system's legitimacy as a fundamental norm, rather than as a tactical position, because rigid party organization did not allow open discussion over the merits of the party's decision (whether tactical or fundamental) to recognize the system's legitimacy. The point here is that just as strategy influences organization, organization influences strategy, and hence must be explored.

The thrust of these studies is relevant to the debate over the behavior of the PCI or PCF in an active role in national government. The question is, of course, whether Eurocommunist parties' respect for the liberal-democratic rules of the game will prove to be simply a tactical guise to be shed once they are in power. The "pure strategist" point of view, which presumes the ultimate flexibility of the party leadership to manipulate the party's position (under the guidance of Moscow), would see little obstacle to this possibility as long as strategic considerations confirmed its feasibility. The authors of this volume are, by and large, characteristically prudent on this issue, since they are well aware that where there is ideological tension, there is unpredictability. But the implication of the empirical findings on party composition, structure, and alliance strategy reported here is that the Communist leadership in both Italy and France must operate under very strong constraints to behave consistently. A reversal in their observance of the rules would seriously strain rank and file support and jeopardize any possibilities of future alliances. Like other institutions that have had to adapt to a particular social milieu, the PCI and PCF are hemmed in by the needs and expectations of their leadership, members, associated institutions (e.g., labor unions) and potential allies. They are anchored in their own routines and traditions, much more so than are the formerly proscribed Communist parties of Portugal and Spain.

Separately, each of these articles would have qualified for journal publication. However, as a collection, masterfully coordinated by the editors, they achieve the most fundamental quality of good comparative research: the study of each system enriches the understanding of the other.

WILLIAM ASCHER

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Southern Africa in Crisis. Edited by Gwendolen M. Carter and Patrick O'Meara. (Bloomington: Indiana University Press, 1977. Pp. vii + 279. $12.50.)

To many of us, it seems an unending and usually losing battle to try to keep abreast of events around the world. Newspapers are a cure almost as depressing as the disease. They lack perspective and seldom help us to interpret events. Geared into reading daily accounts, we become little better than the policy maker who cannot break his addiction to the telex.

More than many regions, southern Africa seems calculated to frustrate the concerned and reasonably enlightened layman. Comparing the variety, pace, and magnitude of change there to that of some other world regions is like comparing *Richard III* with *Waiting for Godot.* We have had a pile of books over the last decade each designed to provide background information and to update current developments. Their purpose has been not so much to report research findings, but to present articulate, soundly analyzed and comprehensive information for the "attentive public" and for non-area specialists. Two immediately come to mind, largely because they have titles strikingly like the book being reviewed. *Southern Africa in Transition* edited by John A. Davis and James K. Baker (1966) grew out of an exciting conference of the American Society of African Culture. Even more extensive, more scholarly and less policy oriented was *Southern Africa in Perspective,* edited by Christian P. Potholm and Richard Dale (1972). Both of these fine books have been overtaken by change.

Now we have a third "in" book about southern Africa, this one edited by Gwendolen M. Carter and Patrick O'Meara, both of Indiana University's African Studies Program. Not surprisingly, the key word in each of the three titles reflects the temper of its time regarding regional affairs. "Transition" (to majority rule) characterized the optimistic liberalism of the early and mid-1960s. "Perspective," less emotional and more reflective, marked the late 1960s and early 1970s when violence seemed to have abated and the white regimes talked of containing black armed struggle. "Crisis" seems ideal for today, especially insofar as the promise of the future is so uncertain. Portugal has been forced out of the region. Rhodesia's whites show signs of defeat. And South Africa has come to know large-scale internal unrest.

A growing American interest in the region (just content-analyze the press if you doubt this) has led the editors of this collection to fashion yet another "backgrounder." The country-by-country organization is neat, but unimaginative. Only Colin Legum's introductory and concluding chapters cover the whole region, even though each contributor takes account of external factors shaping and being shaped by his or her particular country.

Taken as a whole, the substantive chapters are a disappointment. They provide the background chronology for each territory, the essential economic, political and social data (although not in a systematic and comparative format), and they take a swipe at thinking about the future. But by and large I find the essays scanty on interpretation and keen analysis. Often what passes for conclusion is just more data, updated. There are exceptions, however. Elizabeth S. Landis' chapter on Namibia is both committed and bright. Her analysis tends to focus on interesting legal facets of the issue, but at least it has character and spirit. John Marcum (Angola) and Gwendolyn Carter (South Africa) present what we have come to expect of them, sound, reasonable and well thought-out analysis. The essay on Mozambique by journalist Tony Hodges is unduly critical of the Frelimo government and Absolom Vilakazi's piece on Swaziland and Lesotho is apologetic of their governments.

Mr. Legum's speculations on the future leave me unconvinced. In the introduction he states openly that two major predictions can be made: (1) that black power will replace white power throughout the entire region; and (2) that this will occur within the next five or ten years. I have no dispute with his first prediction. But on what basis are we told that it all will happen within a decade? One would have thought that Legum would have learned by experience that to try to set dates on transitions in southern Africa is at best guesswork. I cannot get out of my mind Legum's prediction in 1964 that it would take two or three years for Angola to gain independence, and another year for Mozambique and Rhodesia, "if they have survived that long." Thus by 1966–68 South Africa would find militant black governments on its borders (Colin and Margaret Legum, *South Africa: Crisis for the West,* 1964, p. 5). Frankly, on equally subjective grounds, I think his guess is optimistic.

Far more engaging and plausible is Legum's discussion of three possible scenarios for transition. One is predicated on rising levels of black urban violence and on the "homelands" developing into black bases of power. Seeing this trend of events, the white polity declares a state of emergency, military rule emerges and opts to negotiate, Algerian style. A second scenario is similar, except that the military regime chooses to resist until the end, leading eventually to external intervention by black states with heavy assistance from communist governments. The third scenario, which Legum sees as less likely, has the present government or a military successor pragmatically seeking to preserve as much of the status quo as possible by calling an immediate constitutional conference involving all population groups. A "shared society," whatever that means, is accepted by the whites. This is Legum's preferred alternative future.

If one is looking for an up-to-date (materials appear up to the fall of 1976) treatment of individual states in southern Africa, with sufficient background to make sense of current issues, this book provides just that. It also includes a useful "Suggestions for Further Reading," and some intelligent discussion of future outcomes. It is unfortunate that it was not made available by a commercial house in a less expensive format, for it is far from the usual university press fare.

KENNETH W. GRUNDY

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China from the Opium Wars to the 1911 Revolution. By Jean Chesneaux, Marianne Bastic, Marie-Claire Bergère. Translated by Anne Destenay. (New York: Pantheon Asia Library, 1976. Pp. vii + 412. $17.95, cloth; $6.95, paper.)

Chesneaux, Bastid, and Bergère have written an extraordinary volume. In 400 pages they convey the epic downfall of the Ch'ing Dynasty in simple and easily digestible prose yet do not shy away from the concrete detail so necessary for understanding this extremely complex historical period. This work is the first book in a projected two-volume series covering modern Chinese history from the Opium War to the establishment of the People's Republic (PRC) in 1949. The series is off to an excellent start.

China from the Opium Wars to the 1911 Revolution is actually an amalgam of chapters from two earlier works in French by the authors: *From the Opium War to the Sino-French War, 1840–1885*, and *From the Sino-French War to the Founding of the Chinese Communist Party, 1885–1921.* Surprisingly, however, this multiplicity of authors and cut-and-paste utilization of previous works has produced a well-integrated volume whose quality and style are consistent throughout.

The authors have written a textbook rather than a major research monograph, and they have sculpted their approach admirably for this intended use. The material in each chapter is neatly broken down into sections. Footnotes are almost nonexistent, but each chapter concludes with excerpts from original documents that convey some of the flavor of the period and also provide insights into the attitudes of both Chinese and foreigners who shaped the history laid out in the text. The history itself is broadly conceived, encompassing not only political and military events, but also related developments affecting the economy, social structure, secret societies, and other areas. Changes in the world far from China that ultimately had an impact on the Middle Kingdom are also brought into the story. These numerous strands are woven together, moreover, into a fabric whose shape, texture and patterns are kept clearly visible for the reader.

There are inevitably some minor problems with this text that bear mention. The authors succeed brilliantly, for instance, in blending in the fruits of recent scholarship on such subtle and difficult questions as the economic impact of imperialism on nineteenth-century China. By contrast, however, they do not sufficiently highlight the continuing historical uncertainties about this period. Their somewhat one-sided presentation on the effects of foreign textiles on the native Chinese textile industry (pp. 214–16) provides a good case in point. The few instances where the authors sit back and address directly the historical arguments still in progress (e.g., pp. 231–33, 240) provide welcome relief from what seems in other places to be too "pat" a history. These occasional interludes whet the appetite for more.

Relatedly, in terms of degree of sophistication, this volume runs some risk of falling between two stools. It seems too detailed for a course on post-1949 China that presents a rapid overview of the preceding history simply to set the stage for understanding the People's Republic. At the same time, it might be too superficial to serve as the major text in a course devoted primarily to late Ch'ing China. A somewhat stronger admixture of broad interpretive judgments and greater explicit attention to historical uncertainties might have enhanced the value of this work for both types of courses.

Lastly, this textbook thrusts to center stage the problem of romanization of Chinese names and terms. The authors use the *p'in-yin* system developed in the PRC, while most other books that students will read in the course utilizing this text employ the totally different Wade-Giles system. For those not pursuing a graduate program in Chinese studies, this seems too great a burden. Still, the popularization of *p'in-yin* in recent years by the Chinese dictates that inevitably all students of China will have to pass through a difficult transition period in which even dilettantes must master more than one romanization system in order to read the basic secondary works in the field. It is a measure of the value of this textbook that its use of *p'in-yin* will undoubtedly accelerate our march into this transition period rather than restrict the sales of this text.

KENNETH LIEBERTHAL

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Demands for Social Knowledge: The Role of Research Organisations. Edited by Elisabeth Crawford and Norman Perry. (Beverly Hills, California: Sage Publications, 1976. Pp. 276. $15.00.)

Since 1971, several large-scale studies of organizational factors that influence the activities and products of social science research organizations and departments have been conducted in four European countries—the United Kingdom, France, Denmark, and the Federal Republic of Germany. The studies are funded through a variety of mechanisms but are loosely coordinated by a committee under the label European Survey of Social Science Research Organisation. For scholars interested in history of science, sociology of science, and science policy, these studies should provide both important findings and rich data.

Demands for Social Knowledge apparently is an early effort to apprise the scholarly community of this work; it emerged as a collection of papers from a 1974 Cambridge conference called to review the progress of the studies. The volume is more on the order of a notice to the community rather than a carefully coordinated explication of theory, research designs, and early findings from the studies. The pieces differ greatly in quality, but three of them in particular—the essays by Cherns and Perry, Perry, and Pollak—convey clearly the nature of the study and offer eventual promise of important findings.

Two concerns seem to guide the studies, the one clearly stated but the other implicit in all that is done. The first is addressed to "general aspects of social science research organisation having a long-term influence on the work modes and substantive programmes of operating units" (p. 2). The studies generally trace the historical development of the social sciences in the universities, including the movement toward departmentalization. They look at recruitment of faculty and students, specialization in teaching and research, nature of degree programs, and placement. They also study specialized research units within and outside academic departments—"teams," "laboratories," "federations," "enterprises," and "service organizations," in one typology—with an eye to their goals, structure, and funding. Data used for these purposes are largely descriptive; despite the one-shot survey nature of much of the data, on occasion an author will seek to make developmental statements without benefit of time-series.

But it is really the other concern that motivates the studies. The authors seem united in an uneasy feeling that the corpus of social science knowledge itself is changing (perhaps being less "scientific") because "research activity is shaped by demands for knowledge by social agents other than disciplinary peers" (p. 9). The government, of course, calls the piper's tune. The tune is written in fewer keys of nondirective basic research and more notes of immediate policy-relevant intelligence and advice. Crawford and Perry argue: "if we can show that the demands for social knowledge emanating from governments, in particular, have given rise to structures in which meeting these demands come to represent *organisational goals* (italics in original) then we have a basis for considering whether they have permanently altered the form and substance of social science knowledge" (pp. 9–10).

Ironically, the studies offer only very weak indicators of this central concern. Unlike Cole and Cole, who recently interviewed program officers in American funding agencies, the authors of the European studies have concentrated solely on the recipients of funds, deriving their perceptions of the purposes for which funds were granted and how these affected the recipients' scientific activities. Only Pollak's French study offers data *from the funding agency side* to show how the movement from an annual subvention of funds for unspecified purposes to a major scholar or team has been replaced by contracts for specific products awarded to competitive research performers.

The Crawford and Perry introductory essay details a useful typology of social science research organizations that is capable of generating hypotheses later explored in the Cherns and Perry historical piece and Perry's examination of organizational data from the United Kingdom. Unexpectedly, Perry found that scholars working in teaching departments and university-based research institutes are as likely to want to influence public policy as are researchers at nonprofit and governmental research units. And scholars in non-university settings are as likely to seek production of knowledge "for its own sake" as are scholars connected with universities. Important methodological innovations are also quite likely to originate at non-university research units. Both this study and the others indicate that the rapid growth in European social science teaching departments has not led to a corresponding growth in research capacity; in Europe, as in the U.S., demands for proportionately larger amounts of time devoted to teaching students have competed for the attention of university-based scholars. And much of the hiring has been of people who are strictly "teachers."

Some of the pieces detract from the quality of the others. The Orlans essays is of the post-Watergate hand-wringing genre, suggesting a climate of collusion between contract researchers and unethical governmental leaders, but offering no guidelines about research ethics. Alemann presents some new terminology that reminds us of the difficulty of specifying organizational goals, but his "formula" is basically redundant with that of Crawford and Perry and not so fruitful. Haurum and Friis provide longitudinal data about Denmark but pose no important questions. Agersnap presents a case-study sketch on the ongoing interaction between research and organizational change in an agency, but its relation to the studies is unclear. Finally, Crawford confronts some interesting concerns about the structural determinants of research careers but fails to use multivariate analysis with controls needed to untangle a fascinating anomaly in her data.

One hopes that future reports from the studies will address central issues in the sociology of science and science policy and will fully capitalize on the data base. In the meantime, *Demands for Social Knowledge* documents some major historical differences in the development of social science research resources between countries in Western Europe.

DAVID C. LEEGE

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Energy for Europe: Economic and Political Implications. By Guy de Carmoy. (Washington, D.C.: American Enterprise Institute, 1977. Pp. ii + 120. $3.25, paper.)

This brief book presents an overview of the energy situation in Europe, with particular emphasis on the resources and economic constraints underlying policy decisions. Its five chapters examine, respectively, Europe's energy needs and resources in physical terms; Europe's energy needs in economic terms; the national energy policies of Great Britain, Norway, the Netherlands, West Germany, France, Italy, and Sweden; several energy industries (coal, nuclear, and so on) in Western Europe; and the effects of the international geopolitical context on Europe's energy supply policy. The author presents a generally descriptive narrative of statistical and other materials. His discussion of the energy problems and policies in the country with which this reviewer is most familiar, Sweden, is without factual error.

An underlying theme of the book is that "Europe has blindly followed the U.S. [energy] growth model without having the domestic energy resources to support it" (p. 16) and as a result is now in a difficult situation until energy substitution and energy conservation policies become fully operative. The author does not appear to be optimistic that the national energy policies of a fragmented Europe, further divided by the energy crisis, can respond adequately to the energy policy demands of the near future.

Carmoy is a professor at the European Institute of Business Administration at Fontainebleau and at the Institute of Political Science in Paris. His analysis of the economic constraints facing energy policy makers is assured and relatively detailed. His political analysis, on the other hand, is generally more hesitant, very brief, and with little elaboration of implications. Apparently, his aim has been to suggest constraints facing policy makers without developing the political choices in detail.

The author has drawn his statistical and descriptive material from a range of previously published sources, primarily EEC, UN, and OECD and energy-sector publications, yet for the most part he has pulled them together skillfully and clearly. He also presents a convenient table of European energy measures as a guide for American readers. At some points, however, the presentation is not totally clear. For example, his statement of the book's purpose (immediately preceding page 1) is so brief that it is not a useful guide to the reader. Some of his discussion of economic questions, particularly in chapter 2, is so condensed that readers without some background may find it difficult to follow; this is a weakness in a book evidently intended for readers new to the subject. Similarly, for a brief book, there is a surprising amount of repetition in chapters 3 and 4. The beginning of the conclusion to chapter 4 (p. 92) appears instead to be part of a missing conclusion to chapter 3. In spite of these relatively minor points, though, the author's presentation is generally understandable, within the limits of a 120-page book examining a wide range of topics.

Those seeking a more thorough introduction to the energy policies and politics of various countries (including Britain, France, Hungary, and Sweden) in a more theoretically informed context will find Leon Lindberg (ed.), *The*

Energy Syndrome (Lexington, 1977) more satisfying. On the other hand, students or others seeking a brief, up-to-date, readable, and inexpensive book either for course use or as an introduction to the study of energy in Europe will find this book an appropriate starting point.

ROBERT SAHR

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Surviving Without Governing: The Italian Parties in Parliament. By Giuseppe Di Palma. (Berkeley: University of California Press, 1977. Pp. xvi + 299. $14.00.)

This is an important volume, which merits and requires careful reading. Its significance derives from its many substantive, methodological, and theoretical contributions to the study of legislative behavior, government effectiveness, relations between norms and practices of the political elite, and mass-elite relations in Italy and in democracies generally. The analysis does not shy away from making specific claims, and it supports them with varied forms of data. As a result, there is much to learn from Di Palma, where his arguments succeed but also where they do not.

The volume's central concern is how to account for low levels of government effectiveness. The analysis rests on two related issues: How to define effectiveness and how to link the Italian government's ineffectiveness to a "syndrome" of negative characteristics that describe Italian society and economy. After incisively criticizing several alternatives, Di Palma defines effectiveness as "the capacity of a decisional body to form agreement necessary to sustain decisions" (p. 21). He provides imaginative indicators for this capacity and uses them to provide the best general analysis of legislation in Italy. However, he does not show that the "syndrome" is "but the product of government effectiveness" (p. 5). Indeed, the definition's rationale precludes that possibility. At best, we have a correlation but no causal relationship. Di Palma implicitly recognizes this problem. He rarely attempts to make direct links between government ineffectiveness and the social and economic problems of Italy.

Di Palma explains the low level of effectiveness with a "funnel of causality" composed of a combination of elite and mass characteristics and the relationships among them. The argument is closely tied to the data taken from a large sample of legislators and to detailed analyses of the content and process of legislation. His argument proceeds as follows: (1) The Italian political elite (legislators) are sharply divided over the legitimacy of the decision rules. (2) No one view has dominated. Hence, the parliament has become a "negotial" body, but without the cultural norms to support its operation (namely restrained partisanship). (3) The formal rules of the parliament make it easy to avoid difficult decisions and to enact "spoils" legislation. They add to the absence of consensus on issues and decision rules. In turn, Di Palma ties these norms, forms and practices of the legislators to the "centrifugal" demands of electoral competition: the partisan ties of the legislators and the requirements of success in a "polarized plural" system underpin the absence of elite agreement over decision rules and, hence, the low levels of effectiveness.

From the central argument, Di Palma branches out to cover other topics. Of particular importance are the analyses of *democratic* effectiveness, the PCI's strategy and tactics, and the persistence of the DC's ineffective regime for these many years. The analysis of each branches off from the main thesis and opens new areas.

Two propositions stand at the theoretical heart of the volume: (1) that the legislators are bound by strong and publicly proclaimed party loyalties; (2) that the requirements of electoral competition demand that the parties engage in ideological "outbidding." Questions may be raised about each of the propositions, their connections and their consequences, namely the resulting "centrifugal thrust" that makes elite alliances impossible. The first proposition implies that legislators follow their parties' demands regardless of their personal beliefs. If so, then of what interest is Di Palma's long excursion into their norms concerning the proper functioning of parliament? Won't legislators follow party demands regardless of personal preferences? The propositions are linked by the argument that the parties' electoral requirements are what determine the behavior of the legislators. However, the parties also procliam policy positions and, in the case of the coalition parties, they announce interparty agreements on policy. Thus, if the key determinants of legislative behavior are the parties' desires, Di Palma must specify why two equally visible stands of the parties have such different consequences. As it stands, the statement that electoral claims outweigh legislative ones is a theoretical assertion whose power to tie the argument together may be unravelled in two directions, if not cut completely. The first is that what we know about the renomination process in Italy casts doubt on Di Palma's claim that legislators are accountable to voters and party activists. Position on the ballot is more closely tied to the decisions of national party leaders and the legislators' own local power than the demands of local activists, who are more likely than not to be beholden to the legislators, and the voters. If so, the requirements of electoral competition as seen by legislators need not inhibit their willingness to work with others. The second comes to the heart of the specification's justification, its explanatory utility.

There are several problems with the spatial metaphor that underpins the analysis. The most obvious question to be asked is whether the "center" is anything other than a place to put non-Marxists and non-Fascists. More importantly, the analysis does not distinguish between the goals and claims of voters and party leaders: is it that voters are ideological, receptive to "extreme" appeals, and "pull" the parties to them? Or is it that the parties seeking "space" find positions away from the center, pulling the voters with them? With regard to Italy, the available data do not clearly support either position. More generally, the problem is that the concepts Di Palma uses are not sufficiently exact. Without this precision, it is impossible to locate and explain the positions of the parties on the spectrum. An example of the confusion surfaces in the claims that the "center is occupied by a single large party. . ." (p. 271) and that the last election showed an "emptying of the center" (p. 285). What can be derived from these statements, when the first refers to the DC and the second to votes coming to the DC from the Liberals and the Social Democrats? There is also the question of the PCI's ability to break out of its extreme position. Given the systems logic of the metaphor, one might ask whether it is even possible for the proclamations of party leaders (p. 220) to change the party's place and velocity in orbit. More concretely, Di Palma questions whether recent changes in PCI behavior signal a real change in purpose. His analysis incisively corrects prevailing views concerning the Italian Communists, but the power of his argument is distorted by the spatial metaphor. He poses the question as whether the party is moving to the center or pulling the center to it (p. 233). My point is not to disagree with his analysis of the PCI; I do not. It is to try to strip it of the spatial imagery. If we had an exact model, we would know if a party is moving toward the center or not. We would know how far and how fast, as well. Until we have specific measurements of position on the spectrum, of distance between positions, of movements, of distinctions between parties and voters, we cannot have the predictions that the model requires. Until then, the spatial metaphor provides exactness through labels, not analysis.

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Les Classes Sociales au Liban. By Claude Dubar and Salim Nasr. (Paris: Presses de la Fondation Nationale des Sciences Politiques, 1976. Pp. xiv + 364. 155 F.)

The savage war which gripped Lebanon over the last two years has given rise to a flurry of new books in English, French and Arabic, most of which are journalistic, partisan, and commercial. Two books stand out as serious and scholarly books: Kamal Salibi's *Crossroad to Civil War* and *Les Classes Sociales au Liban* by Claude Dubar and Salim Nasr. While the former book was conceived and written during the course of the war as an attempt to inform and explain, the latter was conceived as a sociological study and researched during Mr. Dubar's two years' stay at the Ecole Superieure des Lettres de Beyrouth, 1971–73. The authors, one Lebanese and one French, trace the social and economic history of Lebanon back to the eighteenth century stressing developments of various groups and their relations. Then they give a very concise and useful account of the Lebanese economy and its transformation in the last two decades. The major part of the book, though, is devoted to a presentation of information on social classes of Lebanon drawn mainly from a survey conducted by the authors in 1973.

Aware that their sample (N = 152) is not fully representative, the authors try to convey the reality of the Lebanese class structure by presenting verbatim 25 interviews drawn from diverse groups and classes. While to some readers this method may seem tedious, it nevertheless provides a rich account of life in Lebanon and a great deal of insight into social and economic reality. Statistical analysis of the survey data is brief and relegated to the last part of the book, perhaps because of the authors' lack of confidence in the sample.

The avowed Marxist framework in which the book is cast is tempered by an open and scholarly attitude and does not prejudge the empirical findings. Marxism in this volume seems like a decorative embellishment and an expression of the authors' outlook rather than a method of analysis. However enlightened, a preconceived ideological position has some drawbacks. Interpretations are occasionally made according to ideological precepts rather than empirical findings. It is pure ideological nonsense to say that because foreign banks transfer some deposits to the metropole, they rob the Lebanese economy and people (p. 71). For these enormous deposits are made by oil-rich countries and do not represent national savings. Moreover, they are far beyond the capacity of the Lebanese economy to absorb or recycle. In their ideological concern with the role of capitalism in the international economic system the authors advocate "economic sovereignty" and an "independent Lebanese economy" (p. 328). This is a dangerously isolationist position and an untenable proposition.

This book is the first major work to address itself to the question of social classes in Lebanon and should be considered a valuable contribution. It remains, however, an introduction to the subject and much more remains to be done. The whole question of applying to Third World countries class concepts developed in Europe during the industrial revolution and after remains fraught with ambiguity, questionable relevance, conceptual difficulties and ideological biases. While the authors of this volume have not been unaware of the problem, they do little to face it. They make no effort, for instance, to explain the limited class consciousness among the respondents, their lack of understanding of class concepts, and the tendency to see themselves as middle-class in a general way. Yet, the country is primarily an urban society, where not more than 19 percent work in agriculture. The authors find that other forces share, if they do not compete with, the class sense of identification, the strongest being sectarianism. But there are others such as various nationalisms, local identification, and group orientation. People interviewed referred to "groups" more often than to "classes." It is not enough to recognize these facts without interpretation, especially in a study with clearly class-based theoretical framework.

A question of central importance, not only in view of the writers' approach but also to the world public, is why has Lebanon turned to civil war two years after the inquiry was completed, a civil war that has been portrayed in the world press as a class war between left and right. *Les Classes Sociales* brings some findings that seriously question such a portrayal. Class consciousness and identification has been shown to obtain among a minority of the population and to be of secondary social and political force. The economy has been prospering and growing at a phenomenal rate in most sectors including agriculture. (The authors' depiction of agriculture as declining reflects nothing but a transformation leading to change in crops and to higher productivity.) The book also shows that unemployment was relatively low (4 percent), and even inflation, of which so much was made, was average in comparison to the world situation (about 13 percent per year between 1967 and 1975). As for distribution of income, the authors rely on the IRFED mission report of 1959 and conclude that there is maldistribution in the system. On all these issues, the authors make their personal assessments partly on common sense and partly on an ideological basis. It would have helped had they tried to be comparative in orientation, especially in describing distribution of income and inflation. Comparisons would have given the reader a perspective as to where to place Lebanon in the world order of things. For instance, the Gini coefficient for Lebanon of 1959 was shown to be 0.53, which is relatively high. The reader would have been able to assess this datum better had he known that the Gini coefficient for France was 0.52 in 1962, 0.51 for Germany in 1960, 0.41 for Sweden in 1963, 0.60 for Tanzania in 1969 and 0.57 for Turkey in 1968. Better still, a regional or world average would have made more sense as a basis for comparisons. All things considered, the question—why a civil war in a prospering society?—remains unanswered.

Les Classes Sociales is a welcome contribution to the literature on Lebanon. It is a must for students of the Middle East and a valuable source of information for students of comparative social structures of developing countries.

ILIYA HARIK

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Brazil Since 1964: Modernisation under a Military Régime. By Georges-André Fiechter. Translated by Alan Braley. (New York: Halsted Press, John Wiley, 1975. Pp. xviii + 310. $30.00.)

Although the social science literature on contemporary Brazil continues to expand, it has not kept fully abreast of the increasing importance of this rapidly developing country. A relatively sophisticated scholar and a cosmopolitan businessman, Georges-André Fiechter has written a book which merits the close attention of those seriously concerned with understanding the process of Brazilian development rather than merely following developments in Brazil. Described by the author as "A Study of Interactions of Politics and Economics in a Contemporary Military Régime," this volume encompasses events through mid-1972 only. Hence it is not, as its title and publication date in this country might lead one to expect, a source for catching up on what has been taking place in Brazil under the present government. Indeed, in a basic sense the chief problem for the U.S. reader is analogous to that which would face a Brazilian picking up an analytic work on U.S. politics written shortly before Nixon's 1972 reelection. As would be the case with such a study of the Kennedy-Johnson-early Nixon years, given a clear realization that a great deal of importance has occurred since, a careful reading should yield a deeper understanding of why these subsequent developments have taken place.

Fiechter, a Swiss scholar who spent some eight years in Brazil, is primarily concerned with what he perceives as "the complexities of a situation which is not readily reducible to the simplicities of ready-made ideological concepts" (p. xiv). Terming the post-1964 Brazilian policy orientation "institutionalized pragmatism," he articulates a personal view approximating that of the more democratically orientated of the moderate elements within the military-technocratic alliance which governs Brazil. In his eyes:

> If opting for technology results in distorting the economy at certain points, so be it. But these distortions must be alleviated by setting in motion a more equitable distribution of incomes, so that by the end of the century the living standard of the rural masses in Brazil may be raised to the point at which, freed from the chains of poverty, they may aspire to a concern with human values and what lies behind them. Such is the objective of plans for development in Brazil; they endeavour to extend interdependence to the four corners of the earth, becoming increasingly impatient of ideological frontiers and choosing with complete impartiality, in each specific case, the solution most favorable to the interests of the nation" (p. xvii).

Finding that during the 1964–1972 period a considerable degree of development—not just growth—had been achieved through an authoritarian framework, Fiechter concludes that:

> Even though the strategy selected has not always met the criteria of intellectual purists as regards distributive justice or democratic liberty, the accelerated growth model has already borne fruit as far as a large part of the population is concerned. Even if it cannot yet be said with certainty whether a sufficiently strong impetus has been given to ensure continued development, it would seem impossible to turn back now. If this growth is maintained, Brazil will indeed become an industrialized nation on the American model, 'The youngest of the giants' (p. 212).

Written originally in French as a doctoral dissertation, Fiechter's book has finally arrived here in an almost prohibitively expensive edition a year after an English translation was first published in London. Yet in light of the increasing significance of Brazil and the present tension between that country's government and the Carter administration, the book still possesses a fundamental timeliness. Over a quarter of the volume is comprised of notes containing not just citations, but a wealth of factual information and insightful interpretations of events. While Fiechter's analysis has generally withstood the test of time quite admirably, the reader should be aware that it ends in midstream, not at any significant watershed or even convenient turning point. In fact, he leaves the Médici government at approximately the juncture the subsequent administration has now reached: the beginning of the succession process. (The term of President Médici continued for another year and a half beyond the period covered by Fiechter, and Ernesto Geisel's presidency will extend until March 1979.) In particular, it is important for the reader to keep in mind the scale of changes in the economic realm since this study was completed. Thus, for example, the 1971 trade deficit of $325 million which Fiechter underscores as the largest since 1947 was to balloon to over $4.6 billion in 1974 under the impact of escalating oil prices brought on by the global energy crisis. Similarly Brazil's foreign debt has reached nearly $30 billion, far above the figure of $6.6 billion he puts forth as an estimate for 1972. Brazil's GNP stands at over $125 billion in current exchange terms, dramatically above the $41–42 billion he cites for 1971. Moreover, the expansion of exports from $1.3 billion in 1963 to $2.9 billion in 1971 which he discusses takes on a quite different perspective when compared to exports of over $10 billion in 1976 and perhaps as high as $13 billion for 1977 (with a single year's growth now equal to the total value only six or seven years earlier).

The basic features of the political system and the developmental model which have emerged under the military-technocratic alliance are still largely as outlined by the author, and herein lies the continuing value of his book. The major political problem of the regime is still to "find means of setting up an adequate organic link between the governors and the governed, to enable it to phase out while ensuring continuity for the method whereby political succession is effected" (p. 208). *Brazil Since 1964* provides the foundation for understanding how Brazil has encountered repeated frustration in this regard during the past few years, while at the same time coping better than most industrialized countries with the international economic crisis unleashed at the end of 1973. Fortunately, by now there are works available to cover the five years of ferment which have ensued in Brazil since the cut-off date of Fiechter's book.

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The Case for the Welfare State: From Social Security to Social Equality. By Norman Furniss and Timothy Tilton. (Bloomington: Indiana University Press, 1977. Pp. xii + 249. $14.00.)

When a political discussion comes around to the issue of what is the best, or at least a better political, social, and economic system, generally the modal response is Sweden. However, those who make such a response, when pressed for elaboration, invariably fail to offer much beyond such phrases as "it's a successful welfare state." The obvious questions—what *is* "a successful welfare state," and whether Sweden fits that characterization—are the primary issues that Norman Furniss and Timothy Tilton address in this study. They attempt to pin down what the "drift to the left" in the West might ultimately look like, and why a welfare state would in fact be a desirable end.

What the authors do, and for the most part do very well, can be broken down into three aspects: (1) they attempt to define a welfare state, in part by distinguishing it from two other modern systems—the "positive state" and the "social security state"; (2) they elaborate on these distinctions by describing the three systems that most closely (but, they stress, not perfectly) approximate these orders—the United States, Great Britain, and Sweden; (3) they defend the welfare state as the best possible social order.

The attempt to define the welfare state is largely a successful undertaking. The distinction between a welfare and a socialist state is not an overwhelming one, but the differences are important (a welfare state is more tolerant of inequalities and diversity and more tentative about the prospects for cooperative social and economic ventures). The definitional exercise is considerably enhanced by the discussion of specific system-types. The differences among the United States, Britain and Sweden are clearly established; given the complexity of modern welfare programs, this accomplishment is no small feat.

To describe these three states briefly: the "positive state" refers to a system in which the government is actively involved in promoting economic growth and stability for the benefit of existing property holders. This, the authors argue, cannot properly be called a "welfare state"—that label is reserved for the other two types, both of which "establish surrogate forms of property for all those without an adequate basis for security and self-development" (p. 2). The "social security state," however, only attempts to provide a national minimum of support. The "social welfare state," by contrast, goes beyond this, aiming for substantially greater equalization than a national minimum would provide, and for substantially greater democratization of political, economic, and social institutions than currently exists in western polities.

The third aspect of this study—the effort to defend the welfare state as the best possible alternative system—is, I believe, the most important and also most disappointing part of the book. The authors are aware that, if a consensus for a welfare state is to be built in this country, it is vitally important to demonstrate that existing welfare states do work. It must be shown that Sweden and, especially, Britain are not as troubled as so many Americans believe, or at least that their problems are not attributable to provisions of the welfare state. Furniss and Tilton make a number of efforts to come to grips with this issue, but they do not completely succeed. Certainly, few can question the list of values (equality, liberty, security, solidarity, and economic efficiency) that they argue would underlie a welfare state, but the evidence that these can be achieved, and without unacceptable costs, is not sufficiently presented. The authors' empirical arguments are too scattered, too short, and occasionally too awkward to be wholly satisfying. It is simply not true, especially if the nonbelievers are to be convinced, that, for example, questions concerning the possible harmful effects of the social welfare state on the economy can "be briefly disposed of" (p. 51). Some nice points (many drawn from Arthur Okun's excellent little book, *Equality and Efficiency*) regarding issues of investment, incentives, and efficiency are sprinkled about in various places, but they need to be brought together in a much more compelling manner. Similarly glib treatment is afforded some other important issues. For instance, the authors suggest that increasing bureaucratization and concentration of political and economic power in a welfare state is not a problem, because the relevant institutions will be "democratized"; obviously, the question of how this might be done needs to be confronted.

Furniss and Tilton conclude by arguing that a consensus can be built in favor of the welfare state in the United States, but that, among other things, it requires the establishment of a solid empirical case. This has not been done in this study. A start has been made, indeed an important one, but with more care and better organization, the case could have been stronger, even with the meager data base that presently exists. The authors' shortcomings in attempting to build this empirical body of evidence, however, only underline the importance of the attempt, and I would conclude by strongly urging that efforts continue in this direction. Comparative policy analysis should not simply be the study of what different systems produce and how they work, but also how *well* they produce and how *well* they work. The search for the best political (and economic and social) system should be at the heart of comparative analysis. It is certainly at the heart of this study and for that reason alone (leaving aside the book's other considerable merits), *The Case for the Welfare State* is an important work.

JOHN M. ECHOLS III

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Allende et l'Expérience Chilienne. By Joan E. Garces. (Paris: Presses de la Fondation Nationale des Sciences Politiques, 1976. Pp. 286. 76 F.)

Joan Garces (he uses the Catalan form of the Spanish *Juan*) is a young French-trained political scientist who came to Chile shortly before the 1970 presidential election and remained to become Salvador Allende's speechwriter and close advisor. He was with Allende in the presidential palace the day of the September 11, 1973 coup, and left at Allende's urging shortly before it was bombed. Franco's ambassador to Chile got him out of the country, disguised as a medical aide, so that he avoided the repression subsequently unleashed against the members and supporters of the Allende government. This is his fourth and most important book on the Allende period. It was preceded by two books published in Chile in 1971 and 1972 on the 1970 election and on the relations between Allende and Congress, and a third published in Mexico and Spain shortly after the coup but largely written before it took place.

The book is important for two reasons. First, it contains a careful analysis from Marxist point of view of the relationship between the revolutionary and electoral roads to power, written by the principal theoretician of the *via chilena* or "second model" of Marxist socialism. Second, it reflects an "insider's" view of the policy debates and choices of the fateful 1970–73 period.

The two perspectives are closely related. Garces concludes his introductory chapter on Marxist theory, "The establishment of a socialist-oriented government by means other than revolution demands not only the unity of the worker organizations but also the alliance and/or political and social coexistence of those organizations with the representative organizations of middle sector workers whether they belong to the government or to the opposition" (p. 33). The rest of the book is devoted to an analysis of the Allende débacle in terms of the basic division on policy within Allende's Popular Unity coalition, and its incapacity and unwillingness to prevent the increasing polarization of the urban middle class and the military into fierce opposition to the Allende government.

Within the Popular Unity coalition the parties were divided on whether to follow what Garces calls the "politico-institutional" approach, which Allende and "up to a point" the Communist party represented, or to intensify class conflict which part of the Socialist party, and the Christian Left, and the MAPU (a splinter group which left the Christian Democrats in 1969 and adopted an increasingly radical position thereafter) advocated. As an example of the latter (and in his view, suicidal) policy, Garces quotes the MAPU position of December 1972 favoring "a great intensification of the class struggle, a polarization of the country into perfectly irreconcilable parties, a split so violent . . . that it always involves the concrete possibility of an armed confrontation" (p. 173). He further notes that in 1973 the initiation of arms stockpiling and training by these groups was immediately detected by the intelligence services of the Chilean armed forces, leading to a generalized belief among the officers that they were likely to be the object of an armed uprising at any moment. In addition, on repeated occasions–in August 1971, January 1972, June 1973, and the week before the September coup–the left wing of the Popular Unity coalition vetoed Allende's proposals for a plebiscite to secure popular endorsement of his proposals for institutional reform against a recalcitrant Congress. At last, two days before the coup, Allende decided to present a plebiscite proposal to the public (after receiving the reluctant support of the Communist party) but by then it was too late. On the day of the coup, each of the parties went its separate way, Allende could not get the Trade Union Confederation to seize the radio stations and right-wing newspapers, and he was left in the presidential palace with 50 civilians and 21 members of his personal bodyguard facing the united armed forces of Chile.

For Garces, the institutional approach was the only viable one given the Chilean circumstances. He believes Allende favored this position down to the day of the coup, but was unwilling to override the opposition within his coalition to the actions which such a policy required. He notes that Allende was overruled when he proposed to the Popular Unity parties that they support a leftist Christian Democrat in the July 1971 by-election in Valparaiso in order to prevent the PDC from moving (as it subsequently did) into an alliance with the Right, and blames what he calls "a vast sector of the left installed in the public administration" for alienating the "non-monopolist" sectors of business and the middle class which could have supported an anti-imperialist policy. What Garces does not observe is that the hostility between the middle class and the Allende government was based, on both sides, on the Marxist ideology and practice of the Allende government which did not agree with him that Marxism could be reinterpreted as a type of nationalist populism allying the middle class and petty bourgeoisie with workers and peasants against the foreign and domestic oligarchs. Such a policy also required an understanding with, or at least the benevolent neutrality of, the principal representative of the middle class, the Christian Democratic party, and cooperation with the PDC was regarded as abandonment of the revolution by Allende's own Socialist party and probably would have led to a split in the Popular Unity coalition.

Most of the books written by those sympathetic to Allende attribute the coup primarily to the now-thoroughly documented CIA program of assistance to opposition groups and media in Chile. Garces devotes one chapter to a summary of the Senate Select Committee report on Chile and refers to what he (erroneously) calls "the ITT-CIA-Frei Plan" to overthrow Allende, but he does not emphasize external factors in his analysis. Each CIA expenditure authorization by the Forty Committee is duly noted in its chronological place. (Garces exaggerates the amount given to the extreme rightist organization, *Patria y Libertad,* before direct CIA aid to it was ended in 1971. The Senate report indicates that it received $38,500 in 1970 and $7000 in 1971, but Garces places the subsidy at $700,000 [p. 122].) Occasionally Garces implies direct causation (particularly in the case of the two political assassinations during the Allende period), but the bulk of his analysis is devoted to internal factors. Furthermore, aside from a graph relating inflation and industrial production, there is no more than passing reference to economic policy, a serious omission given the adverse impact of the runaway inflation on the politics of Chile from mid-1972 until the coup. New material in the book not available elsewhere includes excerpts from Allende's five radio speeches made between 7:55 and 9:30 a.m. on the morning of the coup, a detailed account (probably based on electronic surveillance) of the general staff meeting that led to the resignation of three constitutionalist generals on August 22, 1973, and a careful examination of Pinochet's public positions in the last days before the coup which seems to confirm the Needler theory of the "swing man" who comes aboard at the last minute. (After the coup Pinochet claimed that he had been preparing it for over two months.)

Garces' book places the emphasis in the analysis of the Allende period where it should be placed—on the primacy of internal politics in maintaining the institutional framework of legitimacy which was a precondition for a peaceful transition to socialism. Whether a genuinely Marxist government could have carried out such a transition democratically in a country as "bourgeois" in outlook and social composition as Chile (Allende never received majority electoral support, although he came close in the municipal elections in 1971) is a question which remains unanswered by what is a most important book on the Allende experience.

PAUL E. SIGMUND

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Women: Roles and Status in Eight Countries. Edited by Janet Zollinger Giele and Audrey Chapman Smock. (New York: John Wiley, 1977. Pp. xiii + 443. $17.50.)

Giele and Smock have done an admirable job of doing the near-impossible: in a collection of ten essays they provide important insights into the problem: "Why under some circumstances do women enjoy respect, higher status, and rewards that indicate equality or near-equality with men, and why is their position unquestionably inferior in others?" (p. 3). Included are descriptive essays on Egypt, Bangladesh, Mexico, Ghana, Japan, France, United States, and Poland. In the introduction Giele provides a "theoretical framework for relating modernization . . . to changes in the status of women" (p. 7). Smock summarizes and analyzes data and observations from the various chapters in the conclusion. The country chapters include parallel discussion of the history and culture of each country, and the relation of women to the family, education, employment, fertility, and politics.

The best and worst of the book stem from the complexity of the main theme, equality, and the descriptive nature of the country studies. The authors run into the problem—as do most of us—of distinguishing between differentiation or difference and stratification or inequality. As Giele writes, "Because men and women frequently perform different roles, however, . . . the currency by which status is conferred on them is not always the same. A man may receive power, a woman love. As with apples and oranges, no one can say which is better" (p. 3). This theme, reinforced in the descriptive chapters, helps reveal the possible variety of meanings and consequences of apparently similar phenomena across cultures. For example, in comparing three essays we find recognition of women's sexuality is associated with restricted female social roles in the Muslim cultures of Egypt and Bangladesh. Recognition of women's sexuality was associated with more egalitarian male-female relations in traditional Ghanaian society. Similarly, although most cultures segregate women from men to some degree, especially insofar as employment is concerned, various status differentials or degrees of inequality may be associated with the same level of segregation in various cultures. This book's approach may help cure those who simplistically associate sameness with equality and difference with inequality. The connection is close, but by no means exact.

The difficulty of defining equality shifts the volume's attention from equality as such to comparative status levels and especially comparative restriction of women's "life options." This does not damage the book, but here as elsewhere the term "equality" should be used less promiscuously.

The descriptive format of the essays necessitates skimming over much material; manh times the reader will wish the author could have taken more time to explain the implications of the information presented. This may pose a special problem for students unfamiliar with the countries discussed.

The country studies are compared directly only in the introductory and concluding chapters; all other inferences to be drawn from comparisons are left to the reader. However, these two chapters are quite rich. They provide unity and coherence by making comparisons from the point of view of a number of themes, including cultural tradition, modernization, religion and ideology, and social structure.

One theme notably absent from the analytical chapters is the use men make of women. Perhaps in order to avoid being labeled "polemical" many women who write on women are reluctant to offer explanations that may fall under the general rubric of "male conspiracy." However each chapter includes some indication that women's rights and roles have been variously restricted or expanded because of the needs or desires of the men who hold power over the women rather than because of the needs or desires of the women. Women's options depend in part on whether a man's status is affected by the number of wives or children—especially sons—he has, his wife's chastity or education, or his ability to survive without supplement by his wife's income. Further, although women have been granted increasing access to sources of power, notably education, property, and independent income, they have made few inroads in achieving power itself. Nowhere are women in positions of economic or political authority over men in more than miniscule numbers.

In summary, this book will serve as a very good text for students and an excellent resource for others who are unfamiliar with the comparative study of women's roles and status or who are unfamiliar with countries included in this volume. The historical and cultural sections of each chapter make it particularly helpful to those who teach courses on women to students with little or no background in comparative politics or history.

One final note to the publisher. This is the second anthology on women published by Wiley. The first remains only in expensive hardcover. One hopes, for the sake of those reluctant to have students buy books costing $17.50, the same will not be true of this volume.

VIRGINIA SAPIRO

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Bureaucrats, Politicians, and Peasants in Mexico: A Case Study in Public Policy. By Merilee Serrill Grindle. (Berkeley: University of California Press, 1977. Pp. xviii + 220. $12.50.)

After years of concentration on the "inputs" of politics, many students of Latin America have begun to contribute to a more general trend toward studies of policy-related processes and institutions. Mexico, with its relatively stable authoritarian and bureaucratic system, has been the object of several such studies, and Merilee S. Grindle has added to this literature with a careful, solid examination of the National Staple Products Company (CONASUPO) under the presidential administration of Luis Echeverría (1970–1976).

CONASUPO is a multipurpose government agency which for several decades has engaged in activities ranging from the purchase and storage of agricultural commodities to the establishment of retail stores for low-income consumers. In the first portion of her book, Grindle details how recruitment and authority patterns within this complex organization are structured by the interaction of formal roles and interpersonal alliances, and by the wholesale transfer of personnel which accompanies each new presidential administration. This is followed in the second half by a description of an attempt by the Echeverría administration to reorient the agency toward a more focused, integrated emphasis on "rural development"—a program in which CONASUPO subsidiaries were to displace the "traditional" caciques and intermediaries as the principal source of credit, supplies and marketing services for the subsistence peasantry.

Grindle argues that, with the backing of Echeverría, the general outlines of the new policy were established without significant opposition, since political careers and bureaucratic resources were highly sensitive to presidential preferences. These policies, moreover, did result in a significant rechanneling of public funds and services into the rural areas. The number of new rural CONASUPO retail outlets, for example, grew substantially. Full implementation of the program, however, was frustrated by a variety of factors: subtle pressures from local elites; the lack of autonomous peasant organizations and the resiliency of existing patron-client ties; the bureaucratic premium placed on the avoidance of public controversy; and the six-year limit on the presidential term itself.

This book, which moves over well-paved conceptual terrain, will contain no great surprises for students of Mexican politics. There is, however, some question (one which Grindle herself acknowledges) about just how much an intensive case study can tell us about the shape and direction of the Mexican system as a whole. Grindle's tentative conclusions stress the flexibility of the system—its capacity to make limited adjustments to new demands without major alterations in its elitist and authoritarian character. This is a view consistent with that of many other observers. On the other hand, the boundaries of the case study do not bring into particularly sharp focus some of the significant signs of weakness in the system which, during the early 1970s, were quite visible from a broader perspective. These included strong opposition by the Mexican and international private sectors to the "populist" policies of the Echeverría administration; mass cynicism; guerrilla activities and other forms of decentralized violence; serious problems of inflation, external indebtedness, and recession. Nevertheless, these intellectual trade-offs should not obscure the utility of the insights which can be provided by in-depth case studies, or the skill and thoroughness with which this one was executed. Well-grounded in both general organizational literature and in wider writings on Mexican politics, and making good use of elite interviews, personal observation, and public documents, Grindle's work provides a useful addition to our empirical knowledge of the way in which the Mexican state touches the lives of some of the poorest segments of civil society.

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The State, Administration and the Individual. By Michael Hill. (Totowa, N.J.: Rowman and Littlefield, 1976. Pp. 256. $13.50.)

In *The State, Administration, and the Individual,* Michael Hill assumes a responsibility which few have dared to touch. His task is nothing less than to address the question of the impact of the growth of the state on ordinary people. With the exception of an occasional humanist like Richard Titmuss, all too many discussions of state policy concentrate so heavily on "up there" that they rarely concern themselves with "down here." That the state has taken on more and more tasks in Western capitalist societies is beyond dispute. What is very much open for analysis is the overall impact which this development has had on the quality of everyday life.

Hill's method can be summarized as follows. First, he presents a development of the growth of state power over citizens, covering a vast historical literature without doing it great injustice. Second, he reviews a wide body of materials dealing with the question of access to benefits, exploring the advantages and disadvantages which have resulted from tendencies toward bureaucratization. Third, he argues that the development of professionalism among civil servants has had ambiguous consequences, for professionals often substitute their own conception of their job for the needs of their clients. Finally, Hill concerns himself with the routes open to people to affect the state's behavior toward them: formal redress of grievances, the impact of planning, especially at the local level, and the question of participation raised in Britain by the Skeffington Report. His balanced conclusion is that participation by ordinary people will be empty unless it is accompanied by the acquisition of real power, though taking this development too far could result in disadvantages if it interfered with the efficiency of large-scale government activity.

In his concern with human values and the scope of his discussion, Hill has performed a service for all those interested in questions involving state power. But serious omissions undermine the book's long-term contribution. Specifically, Hill's work suffers from a method which asks the wrong questions, from a lack of attention to political economy, and from a failure to address recent work involving various theories about the role of the state in advanced capitalist societies.

The basic approach of the book is to search for the advantages and disadvantages of various social developments. For example, in his discussion of bureaucratic rigidity, Hill noted that rule-bound organizations allow for predictability, avid corruption, and minimize stigma. On the other hand, such procedures are often slow and unable to account for exceptional circumstances. But something more is needed than a catalogue of the goods and evils of bureaucratization. Why is it that public life in the twentieth century has become so bureaucratized? To what social forces is bureaucratization a response? Writers as diverse as Theodore Lowi, Claus Offe, and Franz Schurmann have all addressed questions of this sort. Their method yields far greater insights into the nature of the relationships between bureaucracies and people than does Hill's.

Related to this is the fact that Hill asks his questions in a vacuum. This is especially evident in his conclusion. When he discussed why community-controlled organizations face difficulties, he says: "But to expect them to achieve freedom from major financial controls, in a society where the central political problems are the distribution of resources between competing claims and the management of the overall economy, is quite unrealistic" (p. 233). This sentence indicates that Hill is aware that the nature of the society within which the state exists is an important concern, but he never follows up the insight. What would be required in order to do so would be some involvement with political economy. How has the response of the state to "the management of the overall economy" shaped its interaction with ordinary people? Not to address this question is to leave the most important cause of the development of state power unaddressed.

This gap is made more serious by the existence of a growing body of literature which does pose the growth of state power as a specific response to the contradictory needs of advanced capitalism. Ralph Miliband's work should never have been ignored in a book of this type. Since Miliband, the development by Jurgen Habermas and James O'Connor of a dialectic between accumulation and legitimation could have helped Hill out of some of his problems, if he had paid attention to it. The role of the state in the accumulation of capital is bound to have a chilling effect on ordinary people, but at the same time, needs for legitimation constitute levers by which people can attempt to subject the state to popular control. This suggests that in advanced capitalism the relationships between state and citizen will be contradictory, filled with tension and incomplete desires on both sides. Such tension is missing from Hill's analysis. The result is a static picture, when a dialectical and dynamic one was possible.

But perhaps one should not criticize an author for not doing what the reviewer would have done in his place. Given his limited objective, Hill's book does constitute a start in fulfilling a crucial need. We do need to know more about how the behavior of the state affects ordinary people, and for calling attention to that need alone, Michael Hill deserves significant credit.

ALAN WOLFE

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Cuban Communism. Third Edition, Edited by Irving Louis Horowitz. (New Brunswick, N.J.: Transaction Books, 1977. Pp. 576. $8.95, paper.)

The more we learn about the revolution in search of modernization and its aftermath, the more idiosyncratic the American Revolution appears. For a brief historical moment, revolution was the tool of successful insurgents who sought release of popular energies in pursuit of development, liberty, and prosperity, and who saw government as part of the problem. In modernizing revolutions since, revolutionaries have ended by using government energetically to direct development, sacrificing resolution of problems in which government itself is the rub. Everywhere revolution has been made in the name of liberty, not just equality and prosperity, in the name of rejecting bureaucracy and the intrusive state, not just social and cultural development. But the great post-American revolutions have bred overweening states, not over time in societies once again settled and stable, but almost immediately. Agonized makers of these revolutions—Mao and Lenin in their last years—have rebelled against their statist handwork but to little avail.

Why this pattern? The history of the Cuban Revolution is instructive. The revolution falls heir to the burdens of sustaining sovereignty in a competitive world, burdens made more onerous as the revolution generates enemies abroad (something the American Revolution largely escaped). Also, those who would now govern find they have not enough government, for enemies must be squelched, the economy managed, the young educated, things planned, people mobilized. Those who now govern find they like it. Finally, it becomes intolerable to leave society alone if its unchecked inclinations will produce development of the "wrong" sort (something the American revolutionaries were confident would not happen).

This book allows us to trace these developments. The Cuban Revolution began life with considerable assets—a relatively developed, though maldistributed, economy and a largely literate population, a cohesive nation, new leaders with remarkably broad support who came to power with little bloodshed or destruction. It pushed through a true social upheaval with dispatch, opening widespread opportunities for the formerly poverty-stricken and exploited, broadening access to social services and redistributing wealth in a context of considerable equality, though the economic base and output have not been greatly enlarged. But it has come up with government aplenty—an enlarging classically structured Communist party, an elaborate army, a spreading state bureaucracy, the omnipresent Committees for the Defense of the Revolution, political prisoners, a socialist caudillo.

For some time Irving Louis Horowitz has given Cuba considerable attention, cultivating an analysis of its development within the tradition of American social science while mediating between that approach and the more perceptive variants of neo-Marxist analyses. His series of readers, of which this is the third, have been effectively broader in the range of analytical postures displayed than most other collections on communist societies. Horowitz's own contributions display the rewards and difficulties of drawing on the two streams of thought simultaneously. His disenchantment with the trends in the revolution is made clear in articles on the militarization of the political system (one of which uses an improbably high estimate of political prisoners). The suggestion that Cuba's situation in this respect roughly parallels that of other Latin military regimes is intriguing. The other articles capture many elements of contemporary Cuba, some by sweeping review and others by penetrating attention to detail. The poignancy of the Cuban Revolution is gradually laid bare—a small country with heroic ambitions caught amidst great states and thus forced into isolation and dependency despite risky efforts to resist; leaders ecstatic at the possibilities of the personal assertion of the individual will ultimately enmeshed in a static Marxism and a statist model of development; an idealistic attempt to diversify the economy and shed material incentives, ground down into a sugar-dominated outpost of traditional Soviet practices.

Difficulties with the collection can be found. The average publication date of the articles is 1972; one wishes for a few more recent treatments. No one suggests that, like Lenin and Mao before him (and, in a way, like Stalin), Castro might revolt against what his revolution has become, which seems entirely possible, given his personality and the authenticity of his revolutionary impulses. There is no thorough assessment of what détente with the United States might mean for the revolution, a very odd omission in view of the traumatic effects of prior détentes on Yugoslavia, Hungary, and Czechoslovakia. Would it decay, or would the revolutionary dynamic be regenerated, once freed from the constraints imposed by Washington and Moscow? Finally, there are elements that provide some basis for understanding the Cuban adventures in Africa but no extended discussion of this remarkable development.

But the virtues of the collection remain, and it may be read with profit by experienced analyst, policy maker, and student alike. More of us would be well advised to do so, as rapprochement between Washington and Havana slowly builds.

PATRICK M. MORGAN

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Nationalism: The Nature and Evolution of an Idea. Edited by Eugene Kamenka. (New York: St. Martin's Press, 1976. Pp. 135. $12.95.)

Eugene Kamenka has edited a useful volume of short essays which succeed in revealing what a treacherous concept nationalism is, and how scholars, no less than other mortals, can be befuddled by its protean forms and emotional power. Kamenka's introduction elegantly finesses the problem of providing an overall definition of his subject. "Nationalism," he argues, "is above all a specific historical phenomenon . . . best understood by examining the specific circumstances under which it arose" (p. 3). His challenge to investigate "the particular character and role" of each specimen of nationalism with full regard for its situational specificity is only partially met by his contributors.

The essays by George L. Mosse and F. X. Martin illuminate the liturgical and mythical aspects of German nationalism and of Ireland's Easter Rising. These are excellent studies of how nationalist symbols are created and manipulated and how the symbols themselves may imprison their manipulators. The essays of John Plamenantz and Wang Gungwu misapply Kamenka's injunction in vain attempts to distinguish, in effect, "good" nationalism from "bad" nationalism. Neither author uses such crude terminology, but Plamenantz's contrast between the "Western nationalism" of Herder and Mazzini and the "Eastern nationalism" of most of the rest of the world comes close in finding the latter "imitative and hostile," "disturbed and ambivalent," and "illiberal, not invariably but often." On one level this is pure Western ethnocentrism; on another it reflects faulty comparative method; what Easterners did is compared with what Herder and Mazzini preached. If what, say, Germans and Italians did were compared with what Gandhi and Nyerere preached, one might reach different conclusions about whose nationalism was disturbed and hostile.

The final chapter, by the distinguished scholar of Hegel and Marx, Shlomo Avineri, similarly misapplies Kamenka's injunction in an ambitious comparison between Israeli and Arab nationalism. Avineri's central contention is that "while Jewish nationalism was relatively successful in imbuing its ideology and praxis with a vision of social transformation, Arab nationalism remained mainly political and by ignoring the social dimension was unable to achieve a degree of social cohesion comparable to the one achieved in the social structures of Israel" (p. 111). Avineri suggests throughout his informative examination of early Zionist and Arab nationalist thinkers that this difference is a matter of doctrine, wisdom, and will. He ignores the specific circumstance that at the time modern nationalism arose Arabs had in place in the Middle East a variety of intertwined social and political structures that have proved tenacious because they are rooted in the needs and interests of real people. Avineri notes that Zionism became "the only migration movement with *a conscious ideology of downward social mobility*" (p. 116). For comparative purposes, his italics are misplaced: it is overwhelmingly important that Zionists did *migrate*. By uprooting themselves from a variety of different social settings they were obliged to create a new social structure. The ideology became important because it was reinforced by specific circumstances which allowed it to flourish and to coordinate the actions of people who lacked concrete alternatives in a new land. By ignoring circumstance, Avineri gives ideology an unwarranted independent explanatory power. He also is led to ignore the closest middle eastern parallel to the Zionists, today's Palestinians who in their own diaspora have undergone major social transformations and begun to build a powerful sense of national identity independent of the Arab political structures of the area, and which in ideology and organization has many points in common with earlier Zionism.

In sum, this is a useful and provocative volume, though hardly a definitive one. Like nationalism itself, it raises as many problems as it solves.

WILLIAM J. FOLTZ

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The Huk Rebellion: A Study of Peasant Revolt in the Philippines. By Benedict J. Kerkvliet. (Berkeley: University of California Press, 1977. Pp. xvii + 305. $16.00.)

Kerkvliet's book is a revised and condensed version of his Ph.D. dissertation, *Peasant Rebel-*

lion in the Philippines (1972). While the dissertation dealt with the Huk rebellion only up to the year 1948, the book also considers the subsequent revolution, i.e., the period 1948 to the early fifties, followed by the decline of the movement in the years 1954–1956.

Kerkvliet used several types of sources: interviews with former Huks, the study of captured documents in government archives, official reports and newspaper articles. He conducted research in a village in Central Luzon (the barrio San Ricardo in the municipality Talavera, province of Nueva Ecija) for a period of four months. During his research he interviewed peasants who had participated in the Huk movement, and he studied local conditions in order to reconstruct the background of the movement. He describes the movement from the thirties to the fifties, and the narration alternates between the national and the local levels. In his description of developments at the local level he uses little vignettes from personal histories and quotations, giving the book a touch of drama as well as of *"couleur locale."* Kerkvliet succeeds in describing the movement from the point of view of the participants, a perspective which has been ignored in previous studies on the Huk movement. The author manages admirably to interweave the sociological analysis of the social configuration with the narration of political and military events.

At the end of the book Kerkvliet presents a number of conclusions, one of the most important of which refers to the causes for and the character of the movement. The author does not locate these causes in such general conditions as poverty, social inequality or high rents, but in the "dramatic deterioration" of traditional patron-client relationships between landlords and tenants in Central Luzon (p. 250). Paternalistic patron-client relationships provided protection for the peasants against crop failures and hardships. These relationships broke down during the thirties when landlords tended to shift to a more commercial approach in agriculture. As landlords ceased to provide tenants with cheap loans, peasants were forced to become dependent on outside usurers. The increasing strictness of the landlords undermined the subsistence base of the peasants. The character of the Huk movement follows from these causes. Kerkvliet states: "Peasant demands were moderate, not radical" (p. 254). The basic objective of the peasants who joined the unions during the thirties and the Huk movement during the forties, was "to regain the economic and political security that the traditional tenancy system had previously provided" (p. 254). The peasant movement was defensive, wanting to stop the trend toward increasing exploitation. It did not push for the overthrow of the government nor did it try to establish a completely different social order. The radicals who advocated land redistribution and a socialist economic order were a minority, according to Kerkvliet. He also depicts the forties and fifties as a period in which a schism continuously grew between the Communist party which tried to gain control of the movement and the mass of peasantry which constituted its following.

Kerkvliet's excellent study, however, does not explicitly consider the class base of the Huk movement and of the repressive forces. Implicitly of course he does. By referring to a history of tenancy for most rank-and-file Huks whose names he has found in documents, he proves convincingly that the Huk adherents and supporters were dependent peasants. Similarly, most leaders also had such a background, although a few leaders came from the landowning class. The author presents a picture of the class structure consisting of two opposing classes: landlords and tenants. One wonders whether social reality can be adequately depicted as a two-class system. There was also a class of small owners-peasants, small traders, shopkeepers and artisans. It would have been interesting to know more about the role of these people during the years of the Huk resistance and rebellion. The question arises whether this social class provided supporters for the repressive forces that have been active against the Huks since Kerkvliet is not very clear about the social background of these counter-organizations.

For example, during the Japanese occupation the Huks sometimes clashed with their rival guerrilla organization, USAFFE, which was affiliated with and officially recognized by the American army. After liberation in 1945 the Americans and the emerging Philippine government tried to quell the Huk movement with the help of the USAFFE. In 1945 and 1946 a counter-revolutionary organization was formed, the civilian guard, out of USAFFE veterans and hacienda guards. These guards were retainers of the landlords and one wonders whether they were recruited from the tenant class or from the middle groups. Kerkvliet quotes villagers from his barrio, who stated that the USAFFE mainly included men who either were close to the government or were hoodlums whom the Hukbalahap had rejected (p. 72). This is possible, even probable, but it does not illuminate the class background of the USAFFE guerrillas or the ensuing civilian guards. In addition, Kerkvliet does not show convincingly that there were no peasants on the other side. It is well known that villages in Central Luzon are raked with factionalism. One might suspect that the landlord-tenant division is not really the same as the Huk–anti-Huk split, i.e., that given the degree of factionalism in villages, one could expect that some of the tenants and some of the peasants were anti-Huk. It would be interesting to know whether landlords could still count on a core of loyal clients among the tenants. The important question is whether the whole peasant population was sympathetic or at least neutral toward the Huk movement, or whether part of the peasant population sided with the landlords.

These remarks do not detract from the fact that Kerkvliet's study is an important contribution to Philippine historiography as well as to the sociology of peasant movements.

W. G. WOLTERS

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Britain Says Yes: The 1975 Referendum on the Common Market. By Anthony King. (Washington, D.C.: American Enterprise Institute, 1977. Pp. 153. $3.75, paper.)

Full-Hearted Consent: The Story of the Referendum Campaign—and the Campaign for the Referendum. By Philip Goodhart. (London: Davis-Poynter, 1976. Pp. 264. £5.00.)

The 1975 Referendum. By David Butler and Uwe Kitzinger. (London: Macmillan, 1976. Pp. xi + 315. £7.95.)

On June 5, 1975, the British people in a popular referendum approved by a 2-to-1 vote their membership in the European Common Market. The political and constitutional implications of this step are of multifaceted interest not only to specialists in British politics and history, but to all concerned with comparative politics. Although the referendum has been used repeatedly in a number of countries—most recently in some to ratify membership in the Common Market—and although it had been seriously considered in Great Britain for more than half a century, nevertheless, it had *never* before been used there on a national basis.

Why did the British take this unprecedented action? How was it carried out? What are its consequences? These are the questions which the authors of the works reviewed here attempt to answer. Considering that these are the first full-length studies of the referendum, the overall result is impressive. Despite the fact that the authors tend to be pro-Marketeers of varying degrees of commitment, their treatment of both sides of the campaign is generally fair and unbiased.

Although the books necessarily overlap and cover similar events, they have different focuses and are most useful for different audiences. *Britain Says Yes* by Anthony King is unquestionably the one which will have the widest market in the United States and can profitably be read by all levels of students. It possesses the wit, clear writing, and sophisticated analysis we have come to associate with King. Since he is concerned, as he states, with explaining rather than describing events, he spends relatively less time with the actual operation of the referendum than with its historical and political background, the motivation of the actors, factions, and parties involved, and the implications of their activities. Thus, not only is the whole story concisely presented, but its relevance and significance are brought out as well.

The 1975 Referendum by David Butler and Uwe Kitzinger, on the other hand, may very well prove to be the most authoritative and fullest account of the referendum campaign itself. Although the work does cover both the historical background and the significance of the campaign, the authors' main concern is upon its actual operation: the procedural mechanics, the composition and strategy of the opposing camps, the conduct of the campaign, and the role of the mass media and the public opinion polls. This wealth of detail is laced with humor and penetrating insight. Since their subject is a political campaign about the Common Market, the authors' expertise is particularly felicitous. Thus, the book may be considered the latest installment of Kitzinger's history of Britain's relationship to the Common Market, as well as a spinoff of the Nuffield general election series in which Butler has played such a prominent role.

Philip Goodhart, a Conservative MP, covers much of the same ground in *Full-Hearted Consent,* but he is much more interested in the device of the referendum, per se. An advocate of the referendum, he gives a thorough account of how it was adopted. In addition, he presents, primarily in the appendices—which contain some of the most interesting material in the book—the gist of an earlier work, *Referendum,* in which he had examined previous British referendum proposals. Although he builds an impressive case that the Conservative party had committed itself earlier in the century to the principle of the referendum, he underestimates the extent of Conservative opposition to the referendum as well as the political expediency of many of its supporters at that time.

Goodhart, on the other hand, depicts with evident relish the expediency of the Labour party's presentation of the recent referendum and the division within its ranks upon both the referendum and the Common Market. In all fairness, the criticism was largely deserved, and none of the other authors misses the underlying ironies in the whole referendum issue, for which Labour, both party and government, was primarily responsible.

The first important irony was that the referendum had been proposed in 1972 by those who wanted to reverse Parliament's decision of the previous year to join the Common Market. They were confident because the public opinion polls clearly reflected the Market's unpopularity with the British people. In 1975, however, public opinion had shifted and the anti-Marketeers were defeated in the actual referendum by a 2-to-1 vote. They thus suffered the ignominy, especially their leading spokesman, Anthony Wedgwood Benn, of being beaten by their own instrument.

The second and more significant of these ironies, in my view, was the contrast between the importance of a potentially fundamental constitutional change and the partisan and casual way in which the referendum was concocted and the unexciting character of the campaign itself. Although previous referendum proposals had also been mired in partisan politics, the distinctive aspect of the politics of the Common Market referendum issue was its intra-partisan rather than inter-partisan nature. Only part of the Labour party favored a consultation of the people on retaining this European connection. The party's Executive Committee accepted it in 1972 by a 13-to-11 vote with its three leading members absent. Although the party's Shadow Cabinet followed suit, it had overwhelmingly rejected the proposal only two weeks earlier. During the referendum campaign, the right-wing Conservatives and the left-wing Labourites were lined up on the anti-Market side against the moderates of both parties, and the small band of Liberals.

There were individuals who opposed the referendum because they felt it would undermine British constitutional principles, especially the sovereignty of Parliament. In assessing the significance of this chapter in British political history, are we to give more weight to the fact that a populist device was used which gave the people the opportunity to exert their will over the political elites or to the fact that they eventually took their cues from their party leaders? Part of the answer to these questions may lie in whether or not the referendum will be used again in the future. The authors point to the proposed devolution of Scotland and Wales as a likely subject. They have described and analyzed the 1975 referendum well; but only time will tell whether it will initiate a fundamental constitutional change, or whether it will be relegated to a footnote in British government texts.

HARRY LAZER

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Oceania and Beyond: Essays on the Pacific Since 1945. Edited by F. P. King. (Westport, Conn.: Greenwood Press, 1976. Pp. xxii + 265 and 6pp. maps. $22.50.)

In 16 solicited essays, editor Frank King succeeds in demonstrating that anthropologists, economists, geographers, historians, journalists, lawyers, political scientists, and public administrators may all share an interest in the vast section of the world referred to as Oceania, and if it is not their declared concern, an undefined political thread binds them all. Beyond this, so wide-ranging are the essays—in subject, time frame, intent, style—that other generalization had best be deferred.

Micronesia-United States relationships account for about one-third of the book—all of Part 1. Individually, these essays range over dimensions of dependency, restructuring of traditional political systems, positive and negative contributions of the Peace Corps, language as a force for unity, and comparable other geographically compatible subjects. Cumulatively, they support the theme of American administrative ineptitude in Micronesia.

Part 2 can best be characterized as a potpourri mainly concerned with Australia and New Zealand, and some of their former colonies. A disconcertingly tangential and stylistically rambling piece places Australia and New Zealand island-related foreign policy in historical and Asian perspective. Independent Samoa's developmental difficulties, Papua New Guinea's search for a foreign policy, and the military defense of New Zealand all raise problems for consideration. A broader essay concerning the state of South Pacific economic development provides a more general view of the island areas in the Australian, British, and New Zealand zones, and the need for political cooperation to achieve economic ends.

If the United States can be treated as the metropolitan nation serving as the focus of Part 1, and Australia and New Zealand of Part 2, France and Great Britain receive their due attention in Part 3. The resistance of France to decolonization of New Caledonia and French Polynesia is balanced with the rapid political change going on in the British areas which has brought one area and shortly will lead most of the others to independence. In addition, the impact of the United States on all of the Pacific Islands comes under review, and—returning to the underlying theme of Part 1—this part proves the ineptitude of the American role in the South Pacific Commission.

Collectively, these essays provide the jumping-off point for the "beyond" referred to in the title of the anthology. Not all of Oceania has been considered, however. Missing is adequate treatment of Fiji and Tonga, Papua New Guinea's internal governance, or the uncertainties underlying the futures of American Samoa and Guam, to name a few. There are gaps, too, in the coverage of nagging regional problems requiring policy resolution—as the Polynesian out-movement for employment, and the "overstayers" in New Zealand. But as an effort at bringing together the contributions of concerned academicians and practitioners from countries both around and in the Pacific, all focused upon Oceania, this volume is not to be faulted. Editor King has pointed the way. The next effort should aim for a narrower delineation of subject, demand a sharper focus, and hold all contributors to the same time frame: after all, "Essays on the Pacific Since 1945" ought to discuss the period since World War II.

NORMAN MELLER

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Judicial Politics in West Germany: A Study of the Federal Constitutional Court. By Donald P. Kommers. (Beverly Hills and London: Sage Publications, 1976. Pp. 312. $15.00.)

Donald Kommers has written the first comprehensive, book-length study in English of West Germany's Federal Constitutional Court, in time for that tribunal's silver anniversary. It is a fitting anniversary gift: thoughtful, generous and without ostentation. It is also a useful introduction of American readers to one of the most powerful and active constitutional tribunals.

The Federal Constitutional Court is empowered, inter alia, to review the constitutionality of legislation, to resolve disputes among members of the federation, to adjudicate the constitutional rights of organs of the national government, to outlaw anticonstitutional political parties, and to settle "constitutional complaints" by citizens who (in strikingly abundant numbers) have felt deprived of their constitutional rights by legislative, administrative or judicial action. In exercising its comprehensive jurisdiction, the court has substantially affected the policies and politics of the Federal Republic and has repeatedly become embroiled in controversy. The struggle over the E.D.C. treaties, the financing of political parties and, more recently, the Intra-German Treaty and the issue of abortion are but a few examples of the involvement of the court in vital and volatile political issues.

Kommers divides his examination of the Federal Constitutional Court into six main sections that deal, respectively, with the political and legal environment of the court ("Setting"); the organization and operation of the court ("Structure"); the recruitment, socialization, background and values of the justices ("Actors"); the procedures, techniques and strategies of the court's decision making ("Process"); some of the principles of interpretation and substantive policies developed by the court ("Policy"); and the effects of the court's work on other actors in the West German political system ("Impact").

While a goodly part of the book is a skillful synthesis of the voluminous German literature that has appeared during the past 25 years, Kommers has included some very useful findings from his own extensive researches on the court. His many interviews have yielded valuable information about the attitudes and values of the individual justices which "are not as yet deducible from judicial decisions" (p. 150). The new practice of publishing dissenting opinions may augur a change in this regard. Equally valuable is the author's detailed analysis, based on his access to unpublished records, of how the court handled a flood of over 1,500 "constitutional complaints" in a single year (pp. 170–75).

Students of the U.S. Supreme Court are likely to find particularly informative the accounts which Kommers gives of some of the internal procedures of the Federal Constitutional Court, such as the practice of assigning one justice as reporter to each case, who, "having to find his own majority," plays a crucial role in shaping the discussion, decision and opinion of that case (pp. 192 ff.); or the practice of "admonitory opinions" in which the court insists that specific legislative action be taken to assure compliance with the constitution (pp. 273–74).

The book is not without faults. The discussion of the "impact" of the court says little about how, or how much, decisions have affected individuals, groups and institutions; indeed Kommers acknowledges that the terms in which he discusses impact are "lacking in elegance of conceptual refinement" (p. 256). Some of the tables in the volume are troublesome; for instance, the incomplete data in Table 14 do not readily support the argument of changes over time in the court's proclivity to void legislative enactments; Tables 6 and 7 are unnecessarily cumbersome; and in Table 19 it is difficult to identify the survey respondents. At times, the author's conclusions teeter precariously at the limits of the evidence, and are then hedged by tentative phrases: "there is reason to believe . . . ; this may not have been so in the beginning . . . ; it is not adventurous to suggest . . . ; the Court seems to be . . . ; nor is it unreasonable to suggest. . ." (p. 265). Also, the tone of the book is rather consistently admiring of almost all aspects of the court (although Kommers does note (p. 246) that "many cases bear the stigma of crumpled logic"), and there is, in the voluminous literature cited by the author, no attention given to some fundamental criticism of the court by such scholars as Otwing Massing, Roland Meister or Wolfgang Abendroth.

Such shortcomings, however, do not seriously detract from the great value of this work. Kommers (and Grossman's introduction) see the book as a building stone for an emerging structure of comparative judicial studies; but while that structure presently seems still far from completion (and design?), this study is a valuable contribution in its own right. It is comprehensive and comprehensible, informative and intelligent. It is a judicious book on an important judicial institution, deserving to be read seriously by students both of German politics and of judicial politics.

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Twentieth-Century Czechoslovakia: The Meanings of Its History. By Josef Korbel. (New York: Columbia University Press, 1977. Pp. xii + 346. $14.95.)

As Josef Korbel states in his preface, his primary purpose is to explain what he calls "the puzzling phenomenon" of Czechoslovakia's history, namely, "a unique zeal for social advancement coupled with an equally unique record of defeat and failure." He begins his pursuit of this rather elusive objective by an examination of "the meanings" of Czechoslovakia's history that "would give it strength to endure ever-changing circumstances or would weaken its will in moments of great national crisis." After having discussed the respective interpretations of Czech history by Palacký, Havlíček, T. G. Masaryk, Pekař, Peroutka and Chalupný, he reviews and analyzes, one by one, the major stages of Czechoslovakia's history from the liberation movement of the First World War to Dubček's "Czechoslovak Spring" of 1968 and its subsequent demise following the Soviet occupation of the country. But the book's emphasis is on the period from Munich onwards.

The book is well organized and written in a vivid and highly readable style. It draws on the most up-to-date materials that have recently become available as well as the author's own knowledge gained in the Czechoslovak diplomatic service and from his participation in the Czechoslovak liberation movement of the Second World War. Appended to the book is a good bibliography of documents, books and periodical literature. One puzzling omission, however, is that neither in the bibliography nor in the footnotes does the author mention the papers of the late Jaromír Smutný, who was the head of the President's Office from 1939 to 1948, even though the papers are deposited in the archives of the university whose press published his book. The book contains also a number of other statements and even direct quotations the sources of which are not mentioned.

While most of Korbel's interpretations and conclusions are, in my judgment, sound and well substantiated, some of them appear to be questionable. Would "the valor of Czechoslovakia" (had Beneš decided to go to war in 1938) have really become "a catalyst to unite the frightened and diffused elements of the West?" (p. 147) Most of the evidence that has come to light since 1938 points in the opposite direction and bears out Beneš' conviction that France and Britain would not have come to Czechoslovakia's aid had Czechoslovakia gone to war, even if Czech armed resistance had lasted the four to six weeks Korbel mentions. Would the nation have "suffered both death and demoralization" any less had it fought and been defeated and overrun by the Nazis in 1938 rather than capitulating? (p. 149) I think not. Could the communization of Czechoslovakia have been avoided had Benes not "bargained away his country's central values?" (p. 216) Again, the fact that all the countries of Central-Eastern Europe that had been overrun by the Red Army toward the end of the Second World War succumbed to communism regardless of their leaders' stand vis-à-vis the Soviet Union seems to indicate that something other than "a compromise of principle for practicality" (p. 216) must have been the real cause. Had T. G. Masaryk stood at the helm in 1938 and 1948, could he have prevented the two tragedies, as Korbel seems to believe? Is it correct to say that "a personal rancor against France and Great Britain played no small role in Beneš' thinking?" Having served as Beneš personal secretary from 1939 to 1945, I can state that the president harbored no ill feelings toward Britain and France as such, but only toward the two countries' "Munichites," Chamberlain, Daladier and, above all, Georges Bonnet.

Although Korbel has striven for scientific objectivity, his book leaves this reviewer with a definite impression that he was influenced in his evaluation of the behavior of Czechoslovakia's leaders at the time of Munich, the 1948 communist take-over and the 1968 Soviet invasion primarily by his own personal conviction that in all the three instances Czechoslovakia should have offered armed resistance. In fact, he even seems to regret that Czechoslovakia emerged in 1918 "from centuries of serfdom into the full splendor of independence—without the firing of a shot, a revolution sui generis" (p. 3).

EDWARD TABORSKY

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Peruvian Democracy under Economic Stress: An Account of the Belaúnde Administration, 1963–1968. By Pedro-Pablo Kuczynski. (Princeton: Princeton University Press, 1977. Pp. xiv + 308. $16.50.)

Peruvian Democracy Under Economic Stress is a work which attempts to provide some insight into economic decision making during a recent period in Peruvian political history. The author, who served as one of the managers of Peru's Central Reserve Bank, played a role in these events in the late 1960s. His work is unique in two ways: first, as a partial memoir, it provides a point of view not normally available to the political scientist examining recent events in Latin America; and second, it contributes to the few studies available of policy decisions in Latin America and in the developing nations.

Although the book focuses on the years 1963 to 1968, Kuczynski provides a brief, lucid background on the social and economic conditions in Peru before this period. Furthermore, in the second chapter, he takes care to describe the individuals (and their political biases) who are to be involved in determining the economic policies analyzed in the following sections. It becomes obvious in this second chapter that Kuczynski's inside knowledge of little-known personnel who had considerable influence on policy formulation provides a much more complete picture than we have had previously. It can also be detected at this point that in spite of his personal involvement in an exciting period of Peruvian history, he writes with remarkable objectivity in his descriptions of personalities. This objectivity continues throughout the book and is equally characteristic of Kuczynski's analysis of the achievements and failures of the Fernando Belaúnde administration. For example, he praises Belaúnde for initiating an inland highway which opened up Peru's interior, while at the same time he criticizes him for his poor choice of economic advisers in the beginning of his administration.

It might seem that the book would only interest a specialist, but Kuczynski has attempted to identify problems and characteristics of decision making to be found in other countries at similar stages of development. In presenting the problems, he analyzes institutional weaknesses in the political structure as well as fallacies in economic policies. As an example of institutional weakness, he argues for changes in the system of proportional representation, which, in his opinion, has been detrimental to congressional relations with the president. The theoretical value of the book to political scientists is lessened by his failure to make the reader fully aware of the broader implications of his findings. While the book is filled with examples which students of developing countries would find enlightening in a comparative perspective, the author himself does not usually expand on their comparability or broader significance. Some of the important themes of general interest to political scientists include: the role of the *técnico* in political decision making; the importance of external influences on domestic policies, in this case, the International Petroleum Company; developmental decision making in a democratic context; and civil-military relations in a period of some political and economic instability.

Kuczynski's text is clearly written and frequently punctuated with humorous anecdotes and asides. Some of the central sections of the book, although maintaining a clarity in style, are somewhat detailed economic analyses. But, again in the final sections, the author intertwines these with political insights. The work concludes with an epilogue devoted to summarizing the achievements and failures of the Belaúnde administration. This section would have added more significantly to the value of the overall work had the author emphasized the broader implications of his study for understanding policy making. For this reason, the book is too detailed for the generalist, but is an excellent work for the Peruvianist and Latin Americanist or for the social scientist willing to ferret out the theoretical implications of Peruvian case examples in other contexts.

RODERIC A. CAMP

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Fascism: A Reader's Guide; Analyses, Interpretations, Bibliography. Edited by Walter Laqueur. (Berkeley: University of California Press, 1976. Pp. 478. $20.00.)

Reviewers rarely have the opportunity of reviewing a collection of essays that is composed of contributions that are almost all unqualifiedly competent. Rarer still are the occasions when such a collection is devoted to as controversial and abused a topic as "fascism." That is why this volume is so welcome. It is surely one of the best single collections given over to a discussion of generic fascism. Juan Linz, Karl Dietrich Bracher, Stanley Payne, Zeev Sternhell, Alan Milward and Eugen Weber deliver themselves of model essays of analysis, exposition and bibliographical reference.

The entire collection that we are considering is evidence of the distance traversed by scholars over the last decade. While the folk-wisdom of social science and history may very well repeat, with tedious regularity, the time-worn convictions that "fascism" was innocent of "ideological" commitments, the essay by Sternhell reveals the complexity and richness of the ideological components of the belief systems we now recognize as "fascist." While "committed" scholarship continues to insist upon the "capitalist" character of "fascism," everything said by Adrian Lyttleton, Milward and Bracher, suggests a far more complicated relationship between industrial and agricultural elites and the fascist parties.

The essays dissipate so many simplisms, so many stupidities, and so many factual and analytic errors, that one can only urge those interested in the general phenomenon we identify as "fascism" to undertake a careful perusal of the book.

The essayists, of course, tender a great many judgments with which specialists may well take issue. Milward, for example, suggests that the Italian Fascist policy of "ruralization"—the effort to reduce the inmigration of rural Italians to the urban centers—was undertaken to placate "threatened," "conservative" supporters (p. 411). An equally plausible interpretation has been offered by Cesare De Seta in his preface to Giuseppe Pagano, *Architettura e città durante il fascismo* (Rome: Laterza, 1976), which construes the policy as the consequence of a very modern preoccupation with the creation of "open cities," with outlying "satellite communities" serving industrial needs, but preserving a "green" and salubrious environment. Furthermore, Francis Carsten's suggestion that "no fundamentally new interpretations of fascism have been put forward by ... modern historians and political scientists" (p. 431) is debatable at best. Rosario Romeo's recent suggestion that Italian Fascism be understood as a stage in "industrialization" and "modernization"—while it shares some features with the early account of Franz Borkenau—is, in a significant sense, a "fundamentally new interpretation" (cf. Romeo's introduction to S. La Francesca, *La politica economica del fascismo,* 2nd ed. Rome: Laterza, 1973). Similarly, there is too much information now available to allow Lyttelton's statement that Mussolini's Fascism was "subservient to the interests of the industrialists and ship owners" (p. 134) to go unchallenged. Since Lyttelton, himself, recognizes that Italian foreign policy, with all its domestic implications, was "exclusively Mussolini's responsibility" (p. 141), it hardly seems plausible that Mussolini was "subservient" to anyone. In this regard, Hans Mommsen suggests that, at best, Fascism's "conservative elites" managed to maintain "some measure of independence" from Fascist dominance—a much more convincing judgment (cf. p. 202). Elsewhere, Sergio Panunzio is identified as "Pannunzio" (p. 333), and the protofascist revolutionary syndicalists are characterized as "right-wing" (p. 332), a characterization that requires considerable argument to render it convincing. Somewhere else, the insistence that the "economic programme of fascism as a sect" had "little relevance in meeting the precise economic demands of those who subsequently supported fascist mass movements" (p. 387) seems to have little merit. Mussolini's emphasis on "productivism"—economic modernization and industrialization—at the very founding of Fascism (as a sect, before it entered into its mass-mobilizing phase) suggests that the program he envisioned was eminently well suited to the interests of the "emerging middle classes," preoccupied with the development of the peninsula. It was that class that was to prove critical in their support of the movement. (Cf. R. De Felice, *Fascism* [New Brunswick, N.J.: Transaction, 1976], p. 45 f.).

All that notwithstanding, Laqueur's collection is a significant contribution to the contemporary scholarship devoted to generic fascism. Taken individually, the essays by Linz, Payne, Sternhell, Milward and Weber, are each, in themselves, ample compensation for the price paid for the volume.

A. JAMES GREGOR

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African Kingships in Perspective: Political Change in Modernization in Monarchical Settings. Edited by Rene Lemarchand. (London: Frank Cass, 1977. Distributed by Biblio Distribution Center, Totowa, N.J. Pp. xiv + 325. $27.50.)

This book seeks to answer an important, if largely retrospective, question about political change in Africa: the extent to which African monarchical systems have been able to "regulate and institutionalize social change and at the same time adapt themselves to the exigencies of their environments" (p. ix). These environments include not only "traditional" Africa before the time of European penetration, but also the more recent periods of tumultuous social and political change introduced by colonialism and national independence.

The book also pursues an important theoretical goal: to provide a corrective to the kind of "uncritical functionalism" that has biased much of the African studies literature toward an assumption of unilinear and irreversible transformations, by returning to an earlier period and "suggesting possible correlations between the character of monarchical institutions and their (variable) vulnerability to social change—while at the same time giving due consideration to the impact of individual choices on institutional variables" (p. x).

The kingships under consideration were selected for their comparative value, according to their different historical legacies, structural-functional diversity, varying levels of institutional adaptability and resistance to decay, and relatively wide geographical dispersal. In his introduction, Lemarchand sets broad analytical parameters for subsequent case studies of the Ethiopian, Rwandan, Burundian, Swazi, Basotho, Bugandan, Ankole, and Ijebu Ode kingdoms. At the time he was writing (1970), all but the Ethiopian, Swazi, and Basotho monarchies had perished. In 1974, Ethiopia too disappeared in a military coup d'état. Nevertheless, Lemarchand argues, these political systems are worth studying because of how they shaped and molded their social environments and thus created many of the institutional conditions and leadership styles that still affect African politics. Because African monarchies and individual rulers displayed variable adaptive capacities in response to the devastating colonial and nationalist experiences, their study can also enhance our theoretical understanding of the relationship between institutional, personal leadership, and environmental factors during such periods of collective upheaval.

Two basic types of monarchy can be distinguished in traditional Africa: "monopolistic," in which political resources were largely controlled by the monarch, and "oligopolistic," where resources were also distributed among other centers of power. Traditional monarchies can also be classified as "open" or "closed," depending upon whether political rewards, including leadership roles, were shared widely within society or were restricted to narrow ascriptive groups. Of the monopolistic kingdoms, the Buganda and Basotho were open, Rwanda and Ankole were closed, and Swaziland and Ethiopia were mixed. Ijebu Ode and Burundi were oligopolistic systems, the former open and the latter mixed.

Still other differences appeared among these monarchies as all but Ethiopia were subjected to long periods of colonial rule. European administration introduced major alterations in the structures of inter-elite and elite-mass exchange, and in the nature and distribution of political resources. These innovations, together with the nationalist opposition to colonialism which they ultimately stimulated, produced widespread transformations in monarchial institutions and highlighted the differing adaptive capacities of the monarchs, expressed in their own polich choices.

Eight case studies follow Lemarchand's introduction. Christopher Clapham discusses Ethiopia as a "theocratic kingship," the twentieth-century survival of which can be attributed to a combination of considerable institutional flexibility, an absence of sustained colonial overrule, and the political sagacity of Emperor Haile-Selassie. Writing in the late 1960s, Clapham pinpoints the reason for the monarchy's destruction in the military coup of 1974: "The immediate future of the emperorship depends on the capacity of Haile-Selassie and his advisors to handle intensified demands" (p. 60), particularly those of a rapid emerging political class comprising students, bureaucrats, and military personnel.

Lemarchand classifies Rwanda and Burundi as ethnically stratified kingships, whose stability and survival depended on rigidly maintained patterns of resource allocation between patrimonial ethnic oligarchies and subordinate ethnic client groups. Colonialism, colonial modernization, and nationalism introduced new political resources into the Rwandan and Burundian systems, with the effect that these patrimonies were disrupted and both kingdoms were violently overthrown. That the monarchy of Burundi was able to outlast that of Rwanda by 12 years is explained by its tradition of institutional flexibility and political compromise produced by "its multiplicity of competitive and semi-autonomous power centres, leading to recurrent (but finally terminal) fragmentation" (p. 94).

As portrayed by Christian Potholm and Richard Weisfelder, Swaziland and Lesotho are ethnic kingships whose institutional adaptability and the innovativeness of their rulers have allowed them to survive into the 1970s. The Swazi monarchy is still firmly in control of that country's government, while the Basotho king now presides over a "symbolic constitutional monarchy"; yet the persistence of both bears witness to the resilience of traditional monarchical forms in revolutionary twentieth-century Africa.

Buganda, Ankole, and Ijebu Ode were "incorporated kingdoms" in that they were absorbed within larger colonial, and then national, political units. Buganda and Ankole, discussed by Crawford Young and Martin Doornbos, became parts of Uganda; while Ijebu Ode, treated by anthropologist Peter Lloyd, was included within Nigeria. The structural and processual transformations of British colonialism rendered Ankole obsolete and an easy target for political and governmental reorganization in the post-colonial period. Through its sophisticated administrative system, its ability to adapt to the colonial pattern of administration, and the personal leadership qualities of its rulers and other elites, Buganda survived and prospered until after national independence in 1962. Its elimination proved to be a costly, and indeed fatal, undertaking for Uganda's first independence government which, with the help of the monarchically disinherited Buganda, was overthrown in a military coup of January, 1971. Under the impact of colonialism, the Yoruba Kingdom of Ijebu Ode simply degenerated into a series of confrontations between the king and his lesser chiefs, and somewhat later between both and the newly emergent mass political parties.

In his concluding essay, Lemarchand evaluates the ability of African monarchs to regulate the massive political changes set in motion by European colonialism. In diverse settings, some kings were better able than others to establish relatively successful strategies of institutional autonomy from colonial and nationalist power centers, and thus develop viable strategies of personal survival and institutional growth. These strategies seem only to have postponed the demise of the surviving kingdoms. Yet their identification and documentation provide the chief contributions of this book to a broader understanding of political change, by underlining the importance of leadership choices to impersonal processes of institutional decay and transformation. For two decades at least, structural-functional analysis has virtually ignored these personal factors, and the present volume helps to correct this lapse.

This value is mitigated only by the volume's high price, which will probably limit its circulation to specialized researchers and libraries, and also by the fact that it was published more than seven years after most of the case studies were completed. Each study would have been improved if its author had added a section updating the experiences and consequences of the monarchy he examined.

RODGER YEAGER

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Chinese Shadows. By Simon Leys. (New York: Viking Press, 1977. Pp. xvi + 220. $10.00, cloth; $2.50, paper.)

Pierre Ryckman (writing under the name of Simon Leys) makes some shrewd observations on the privileged position of the military sector in Chinese society in his book, *Chinese Shadows* (pp. 46, 68, 82, 83). Cultural historians may, in addition, find in the work a useful compilation of some of the destruction wrought by the Red Guards during the Cultural Revolution. Sadly, the book fails as a deep analysis of Chinese politics. Its attempt to explicate the real roots of China's political dynamics results only in confusion, contradiction and, most unfortunately, McCarthyism.

Ryckmans contends that virtually all other professional analysts of Chinese politics are fools, flatterers, frauds and apologists. The truth that all others but he supposedly miss is that China is a cretinized, totalitarian, Stalinist, slave society of ant-like people. Ryckmans is not given to understatement.

Since Ryckmans believes he has nothing to learn from academics who do not share his simplistic unidimensional explanation, he never comes to grips with the professional consensus which finds more utility in complex non-totalitarian paradigms related to comparative communism, conflict models, nationalism, interest-group theory, bureaucratic politics and notions of collective action, techno-logic and modernization.

Ryckmans' observations, on contrast to his theory, are more in accord with these non-totalitarian paradigms. He records the obvious facts such as Mao's inability to impose his will on the bureaucracy, the almost permanent factions, the nearness of military coups, the persistence of virtually feudal traditions, the vitality of family and the ease with which some educators got around reforms they didn't like. And he does not invent a secret police which terrorizes the populace, since there is none. Indeed, he reports his observation that workers fearlessly impose their own rules.

Ryckmans then ignores what he observes in the present and proceeds to find the roots of his fictive Chinese totalitarianism in China's past. Supposedly, Maoism grows from and builds on a hypostatized totalitarian tradition instituted by the Ming dynasty some 600 years ago. At the same time, Maoism allegedly destroys the continuing humanist Confucian tradition which long predated the Ming dynasty.

This gross and distant argument which finds the origins and meaning of contemporary Chinese political dynamics in ancient times makes no sense at all to me or to most other political scientists of People's China concerned with party and ideology, modernity and socialism. And Ryckmans has no other explanatory scheme to offer.

Ryckmans' good Confucian past even includes traditional China as a veritable "two party democracy" (p. 209). Meanwhile, the real populist, localist, decentralist facet of Maoism, analyzed by so many scholars, receives not a word. Instead, Ryckmans argues that a supposedly sybaritic past (pp. 37, 50, 80, 193) has been replaced by a leveling-down. Apparently, he alone never heard about China's earlier mass starvations.

Despite Ryckmans' unique conclusions about the happy past and sad present, the historical evidence points to a famine-filled recent past replaced by what Western economic historians of China find to be an extraordinary raising of the worst-off regions and a successful closing of the economic, social and cultural gap between the poorest rural areas and the wealthier urban ones. Since the bases of Ryckmans' theories conflict at every key point with the professional consensus, it is no wonder that he damns and ignores the contributions of professionals.

In fact, Ryckmans' own evidence is not even congruent with his own theses that most everything is wonderful in the surviving elements of the Confucian past. Evincing no love for the dogmatism of much of Chinese educational methods, Ryckmans' comments that "Chinese pedagogy remains . . . desperately conservative and traditional" (p. 147). In addition, he notes that the "age-old contempt of mandarin society for manual work" persists, as "city dwellers still look down upon a countryside posting as exile or disgrace" (p. 48). One less biased than Ryckmans might almost find in his observations some small justification for Maoism's belief in China's need to undermine some undemocratic aspects of Confucianism.

If the data on Chinese politics and society won't support his thesis of a monstrous present replacing a magnificent past—he dismisses the evils of the past as mere "historical accident" (p. 219), Ryckmans tries to carry the day with an extraordinary verbal assault of extreme language. Words such as "atrocity," "rape" and "murder" (pp. 160, 53, 143) and comparisons with the world's most notorious conquerors and killers are linked to the most ordinary acts of China's modernization such as the removal of city walls, the widening and straightening of streets, clapping at the conclusion of a performance and language reform.

No comparison is ever made with other nations' modernization or earlier moments in China's modernization, although that might permit a serious evaluation of the costs and benefits of China's path. It really isn't serious analysis to idealize China's distant past and demonize the present as Ryckmans does.

This demonology combines two theodicies. First there is the reverence of antiquarians and former colonials for the heavenly old days in China. Ryckmans pines for some imagined traditional China with its supposedly "straightforward," "warm-hearted," "simple," "natural," "naive," "courteous," "crafty," "subtle" people. You know, the "patient" ones such as the "devoted" cook of the foreigner (pp. 51, 186, 187).

Second, Ryckmans describes himself as one who once adored the revolution as good and now finds it hateful (pp. 86, 159, 192). The flip-flop not only produces the expected percentages of emotion (maximized) and reason (almost nonexistent), but also the major metaphor of the book: the revolution as a false god.

As a result, the book is full of one major judgment or error after another, each and all characterized by the victory of blinding venom over balance or fact. Whereas respected observers such as Benjamin Schwartz and Wang Gengwu describe Mao as an accomplished poet whose poems stir something in every literate Chinese, Ryckmans has only vile adjectives to describe that poetry. Whereas all Western biographers of Mao are grateful for Li Jui's *The Early Revolutionary Activities of Comrade Mao Tse-tung* (recently translated into English), Ryckmans insists that the Chinese are kept completely ignorant of Mao's biography (p. 83). Whereas Mao wrote reams in criticism of Stalin's way (an English translation of some of this is titled *A Critique of Soviet Economics*), Ryckmans insists that not one such word exists (p. 203).

What we have from Ryckmans, I fear, is not ordinary ignorance or error, but pure malice. His analysis is at this level: "one can see what is wrong with the left-wing movement by the ugliness of their women" (p. 196).

Worst of all, the book is unforgivably vicious. Typically, Ryckmans begins and ends with a back-handed attack on Harvard's John King Fairbank (who probably contributed more than any other individual to fostering rigorous American scholarship on modern China) as a flattering advocate of Maoism. When I recall the hurtful slanders of Fairbank in this regard in the McCarthy period, when I remember that Fairbank's basic category for People's China is totalitarianism and that he has argued at length to keep Taiwan from the PRC, I conclude that Ryckmans is so much out for effect that he has forgotten not only facts and history but basic human decency.

EDWARD FRIEDMAN

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Authoritarianism and Corporatism in Latin America. Edited by James M. Malloy. (Pittsburgh: University of Pittsburgh Press, 1977. Pp. x + 549. $19.95, cloth; $5.95, paper.)

The rediscovery of the concept of corporatism has given a certain elan to the study of Latin American politics. It has provoked a lively debate and stimulated a considerable program of research that is now spreading into other areas of comparative politics. Stripped of its associations with Fascist ideology, universalized, operationalized and embedded in a context of important theoretical concerns, the idea of corporatism does have implications for the study of political institutions and processes in the broadest sense.

There is a rough agreement that corporatism signifies a specific pattern of relations between the state and other social institutions and associations, one in which government plays the role of architect of political order, defining and delimiting the scope of interest group activity, coordinating the activities of private associations, creating explicit mechanisms of direct interest representation in public policy making. Unlike orthodox pluralist theory, the state is not seen as "responding" to demands autonomously generated by groups. The state is an active force consciously imposing design and structure on group process. Unlike totalitarianism, corporatism does not entail the comprehensive mobilization and politicization of society.

Beyond such rough agreement, corporatism means different things to different people and can be understood at a number of different levels. Hence the lively debate. In one sense, corporatism connotes the political world view of the Aristotelian-Thomist synthesis and medieval thought. It is a general pattern of political cognition like liberalism and Marxism. One school contends that this political perspective is uniquely embedded in Hispanic culture and that a proper political interpretation of Latin America must start from these constructs and assumptions rather than those of American political science or Marxist analysis.

A second view takes corporatism as a general model of the political system with no particular cultural or ideological ties or connotations. Corporate institutions and practices can be identified in Northern Europe and Anglo-America as well as in other parts of the world. Corporatism is a model like pluralism or totalitarianism, part of a taxonomy of comparative political forms. The analytic task is to clarify its character and to account for its incidence.

A related group of scholars see corporatism more as a characterization of policy than system. It is the intent of the state to create a certain structure of relationships between associations and the state that is the object of analysis. Finally, some simply associate corporatism with a certain kind of authoritarian-technocratic government.

The volume edited by James Malloy is an important contribution to this growing focus of research. First, it must be said that the book is remarkably well edited and organized for a multi-authored work. We begin with four theoretical pieces, move to ten comparative and case studies and conclude with an exercise in cross-national comparison by David and Ruth Collier and a fine critical summation by Silvio Duncan Baretta and Helen C. Douglass. There is even a bibliography.

The specific focus is on the utility of the concept of corporatism in analyzing the "modernizing authoritarian" regimes that emerged in Latin America in the late 1960s. I found no single, brilliant piece in the book—nothing to match Phillippe Schmitter's seminal "Still the Century of Corporatism?"—but taken together, the articles are craftsman-like and do illustrate the range of research interests, techniques and approaches that have been arrayed around the central concept.

James Malloy and Guillermo O'Donnell try to put the phenomenon of authoritarian corporatism in developmental perspective. Schwartzman contributes a nice essay relating corporatism to Weber's theory of patrimonialism. Chalmers speaks to the "politicization" of the state.

Case studies include articles by the Purcells and Stevens on Mexico, Bailey on Colombia (a most sensible piece), Miricle on Brazil, Sharpe on the Dominican Republic, Dietz and Palmer on Peru and Malloy on Bolivia. Most are successful on carefully defined problems. Kaufman tries to draw party competition into the corporatist net in a comparative study.

I want to single out Thomas Skidmore's piece on economic stabilization because it seemed curiously unrelated to the theme of corporatism and I think I know why. Skidmore examines the policy failures of a variety of Latin American regimes in coming to grips with inflation. He sees economic policy—much as economists do—as having to do solely with macroeconomic management, with fiscal, budget and monetary policy instruments. However, corporatist institutions, structured collaboration between labor and industrial associations and the state, are alternative mechanisms of economic management and particularly of incomes policy. European experience in the recent past may suggest that a positive collaboration of such associations with the state is one of the few keys to workable stabilization policy. That Skidmore did not see the link between the theory of corporatism and economic policy signals an interesting gap in theory, one that is significant for the development of a more coherent approach to political economy.

The intellectual history of the study of corporatism in political science is relatively brief and coherent. Anyone who would take up Pike and Stritch's *The New Corporatism*, skim the April, 1977 issue of *Comparative Political Studies*, and then read the Malloy volume can claim to be reasonably familiar with the subject, perhaps even a minor authority in the field.

CHARLES W. ANDERSON

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The Rise and Fall of the Cyprus Republic. By Kyriacos C. Markides. (New Haven and London: Yale University Press, 1977. Pp. xvii + 200. $12.50.)

Markides, a Cypriot sociologist, explores the social dynamics of the Greek Cypriot community which made possible the external interference that brought down the Cyprus Republic in 1974. His analysis is based on current theories of social transformation, political accommodation, and the breakdown of democratic regimes.

The more interesting and potentially controversial sections of this book are those dealing with Makarios' authority and power and the social and political characteristics of his supporters and opponents. Markides argues that Makarios' authority and power is based on the rare combination of traditional, charismatic, and legal-rational authority, with the charismatic overshadowing the other two. His charismatic authority is reinforced by the control of the key economic institutions of the republic. In contrast to other charismatic leaders, Makarios has maintained and strengthened his charismatic authority and has enjoyed wide support from all social and political strata, thus dominating Cypriot politics since 1950.

In the analysis of Makarios' authority and power one finds both an implied criticism of the Cypriot leader, as well as an expression of concern about the future of Cyprus. In the former case, the author argues that Makarios' charismatic leadership prevented the emergence of viable political institutions and alternate leaders that could have paved the way for a settlement of the perpetuated Cypriot dispute. Makarios' charismatic authority and role as unifier in a fragmented social and political system made him indispensable, despite his inability to resolve the "insoluble" problems of intercommunal relations and the issue of *enosis*. This assessment will be welcomed by Makarios' critics, in and out of Cyprus, who have maintained for years that Makarios is a "crisis leader." But the conditions leading to Makarios' indispensability also create concern as to the future of Cyprus, a concern confirmed by the recent heart attack of the Cypriot president.

The presence of the previously mentioned "insoluble" problems led to the breakdown of the Cypriot Republic in the summer of 1974 by contributing to the government's loss of legitimacy, increasing the scope for terrorism, and fragmenting the governing coalition. Thus the government's attempt to deal with the activities of the Athens-led "disloyal opposition" was "too little too late," making the 1974 coup inevitable.

The discussion of Makarios' power and authority and the effects of his charismatic rule is thorough, as is the author's analysis of *enosis* and its effects on the problem of Cyprus. But Markides' assessment of the linkage of the internal and external factors in the Cyprus dispute and the problems of charismatic leadership raises a number of questions. Approaching the Cyprus problem from an international perspective available documentary evidence indicates first: that the policy priorities and actions of Greece, Turkey and the U.S. made the post-1963 clash in the objectives of Greece and Cyprus inevitable. Such a clash may have been avoided if other Cypriot leaders, such as Clerides or Grivas, or even Makarios, either led Cyprus to partition under the guise of *enosis* schemes (such as the Acheson Plan; Grivas-Ball agreement; Toumbas-Caglayangil protocol) or acquiesced to the confederal-partitionist demands Greece and Turkey presented at the Cypriot intercommunal talks following the understanding the two countries reached in Lisbon in 1971. Second: the attitudes of the Greek junta toward the Cyprus dispute and the Cypriots in general support the argument that the coup in Cyprus would have occurred regardless of Makarios' actions. And third, the author's suggestions as to policy options that Makarios could have pursued are either unrealistic (resignation), or attempted and met with rejection by the U.S., Greece and Turkey (alignment with the West and NATO).

The author's attempt to apply the theory of "consociational democracy" to the problem of ethnic accommodation in Cyprus is useful, but one wonders why Nordlinger's contribution on the subject was overlooked. Although he refers to the intercommunal talks of 1968–74, the author fails to show that the negotiators had produced a new formula for conflict regulation in Cyprus just before the July 15 coup. This fact largely invalidates the thesis that Makarios could not resolve the intercommunal problem and indicates that Athens would have overthrown his government unless the Cypriots had accepted externally imposed schemes. Finally, the book is replete with spelling errors including the names of important participants in the Cypriot tragedy (for example, General Gizikis).

Despite these drawbacks, the book is recommended as it is indeed a major attempt at studying the internal dynamics of the Greek Cypriot community that made external intervention in Cyprus possible. The book should be of interest to a diverse audience including those interested in ethnic conflict and transnational politics.

VAN COUFOUDAKIS

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The Revolutionary Ascetic: Evolution of a Political Type. By Bruce Mazlish. (New York: Basic Books, 1976. Pp. vii + 261. $11.95.)

Most who have studied the history of modern revolutions comparatively have encountered a basic problem: do many or all the top leaders betray common personality traits that substantially account for their political behavior? In short, is there such a thing as a "revolutionary personality" at the elite level? Only the dogmatist will answer one way or the other with full confidence and few reservations. Bruce Mazlish in *The Revolutionary Ascetic* has his own answer, which differs somewhat from the earlier answers of Gustav Bychowski's *Dictators and Disciplines* and E. Victor Wolfenstein's *The Revolutionary Personality*. Mazlish's answer is that there is no revolutionary personality. It is perhaps the professional historian's greater caution (or timidity) in generalization—in contrast to the approach of the psychiatrist or the political scientist—that explains his conclusion that the theory of the revolutionary personality suffers from "grossness" and its "utter simplicity in the face of the multiplicity of actual revolutions, with the resultant emergence of varied sorts of leaders and followers" (p. 3).

This conclusion has often been reached before, but when it has it usually coincided with an overall rejection of the methodology of "psychobiography" or "psychohistory" with its roots in the psychoanalysis of Freud, perhaps as mediated by Erikson and others. No one familiar with Mazlish's earlier work, however, would expect him to embrace such a drastic and pessimistic conclusion. Indeed, his rejection of a generic revolutionary personality is linked both to his rejection of psychobiography and his acceptance of psychohistory.

Psychobiography would involve a no-holds-barred Freudianism which, applied to revolution, would explain the leaders' revolutionary behavior in terms of unsuccessful coping with the Oedipal conflict, perhaps punctuated by a need to repress "homoerotic libido" (Bychowski). Psychohistory, on the other hand, would employ the conceptual tools of Freud, Erikson, Lasswell and others more circumspectly and try to integrate the properly psychodynamic factors with those stemming from the social and historical milieu. Psychohistory would exclude putting all top revolutionary leaders in the same mold, but leave open the possibility that some important revolutionary leaders share clear and distinct personality traits. If these traits do not exactly constitute a "syndrome" (i.e., a mandatory list of necessary conditions), they make a set of "family resemblances" that no researcher can afford to ignore.

Influenced by the above methodological concerns, Mazlish's more substantive thesis is that the revolutionary ascetic represents a genuine political type, exemplified rather well by such as Cromwell, Robespierre, Lenin, and Mao Tse-tung. (Mazlish also alludes briefly in the preface to contemporary figures such as Yasir Arafat and Colonel Qaddafi as possible candidates.) He defines the "ideal revolutionary ascetic" as "one who has identified his self-love with an abstraction, and is also able to avoid counter-transferences to his followers and to control them by means of 'suggestion.' He is also 'ascetic' in the more usual sense of the word: that is, he abstains from 'wine, women, and song" (pp. 28–29).

Thus the revolutionary ascetic professes love for the people, the masses, or the proletariat, while his capacity to establish cordial relationships with actual people may be quite limited. Instead of being purely narcissistic, however, he has fallen in love with the revolution or its ostensible beneficiaries. He has sublimated libidinal drives and diverted them to the realm of political action.

To this point Mazlish's explication of the revolutionary ascetic relies on Freudianism and its offshoots. However, in a synthesis that involves further methodological problems, he wishes to bring in Max Weber's notion of "worldly asceticism," as developed in *The Protestant Ethic and the Spirit of Capitalism.* Mazlish tries to show that the wordly asceticism stimulated by Calvinism had not only economic consequences, but also political consequences in the growth of revolutionary politics. He thus analyzes its transit from the religious through the economic to the political modes. As the ideal worldly ascetic capitalist transfers some of his excess energies to business, the revolutionary ascetic diverts his to radical politics. Moreover, since this asceticism is extraordinary, it can serve as the mark of that heroic, superhuman quality that Weber associated with charismatic leadership. Asceticism not only motivates the leader, it also gives him mastery over his followers.

The Revolutionary Ascetic is divided into three parts. Part 1 is the theoretical section. Part 2 deals with the "historical stages" where Mazlish traces the evolution of his type from the Reformation through the English and French Revolutions (Cromwell and Robespierre) and up through prerevolutionary Russian history (Chernyshevsky and Lenin). Part 3 has a case-study chapter each on Lenin and Mao, plus a conclusion. This book does not claim to be a definitive statement. Yet it combines a salutary historical caution with enough theoretical daring to carry the discussion about personality and revolution to a new stage. The psycho-biographical quest for a revolutionary personality may be a blind alley, but the psychohistorical analysis of the revolutionary ascetic points toward other possible applications.

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Housing and Race in Industrial Society: Civil Rights and Urban Policy in Britain and the United States. By David H. McKay. (Totowa, N.J.: Rowman and Littlefield, 1977. Pp. 193. $13.50.)

Racial discrimination in housing in both the United States and Britain has long been a serious problem, but both nations have preferred to ignore the issue, or if action is taken it usually has been ineffective, limited, and without important consequences for the bulk of victims of discrimination. Indeed experience indicates that antidiscrimination legislation is designed to appear to be addressing the problem while in fact doing little or nothing to modify racially discriminatory housing practices. Loopholes are left in the laws purposely to minimize their impact. For example, an agency is established to administer an antidiscrimination law, but barriers are fixed so the agency will not achieve much. The agency may be woefully understaffed, or it may be required to proceed on a case-complaint basis resulting in, at best, the solutions to problems of specific individuals, but making no dent in the prevailing and widespread discriminatory housing practices. There are other ways to hobble administrators of these laws. An agency may be denied authority to subpoena information, to issue cease and desist orders, or if challenged on appeal, the agency loses jurisdiction and a court begins proceedings de novo, guaranteeing long delay and minimal results.

Despite the evident limitations of antidiscrimination laws (or perhaps *because* of their limitations) they have been enacted broadly in the United States and more recently, in a restrained fashion, in Britain. Accordingly, a considerable literature concerning these laws and their enforcement has developed. David McKay of the University of Essex in England reviews this literature and provides a comparative analysis of racism as it affects housing in both the United States and the United Kingdom.

There are of course marked similarities, as well as differences, between the countries in matters of race. The minority population in Britain is small (about 2.5 percent or 1.4 million people), and in the U.S. blacks alone (not counting other minorities for the moment) comprise at least 11 percent of the population (23.4 million people). And the two nations' historical experiences with racism are significantly different. Britain did not have to go through the process of eliminating legal inferiority (i.e., never having legislated segregation, they did not have to struggle to achieve legal equality which was at the center of the civil rights movement in the U.S. during the 1950s and 1960s). Moreover, the British, perhaps even more than Americans, have told themselves that racism did not exist, yet, as McKay demonstrates, in countless ways the language and behavior of white Britain reveal deep-seated prejudice to migrants from the commonwealth. They have even pretended that curbing this migration had nothing to do with racism.

Nevertheless, there are obviously many similarities between the two countries in their politics of race. Knowing that blacks and other minorities frighten many Britons, politicians have played on those fears for political advantage. Also common to both countries is the process by which seeming "solutions" to racist problems are offered—in full knowledge that they are worthless. (Indeed, as I have said, it has often been true in both countries that the laws have succeeded in passage only *because* they were worthless to effect real change.)

McKay discusses the inevitable issue that arises in making comparisons between the U.S. and the U.K.—centralized versus decentralized governmental systems. He sensibly observes that contrary to arguments commonly advanced for the superior effectiveness of a more centralized government, unitary government is no assurance of effective programs against racism. Despite centralization, local discretion on housing policies persists in Britain and discrimination results. But, says McKay, "it would be misleading to imply that Britain's centralized system, reformist tradition and extensive public sector are of little significance. Clearly, these factors have influenced policy and *do* provide greater potential for change than does the American system characterized by political fragmentation and limited government involvement in society. . ." (pp. 182–83). When it comes to political action, however, McKay rightly observes that the "more favorable" structure is no assurance of action. More crucial is the issue, its history, and the power of the groups and classes involved.

In his final observations, I think McKay rightly chastises most American writers on race and housing, accusing them of a kind of Pollyanna-like desire always to end on a hopeful note. This, he feels, is often based upon "the vain belief that community control and participation are the way to reduction of racial inequities" (p. 188). What these scholars overlook, says McKay, is "the overriding importance of investment in industry, commerce and social infrastructure as a determinant of the economic, political and social opportunities of the residents of depressed inner city areas" (p. 188). Since in neither country have poor blacks much opportunity to control investment policy, there is therefore scant reason to expect resolution to inner city housing problems. Alas, McKay's pessimism seems well founded.

All things considered, McKay has done a competent job of presenting an informed review of the housing conditions in the two countries and of the politics of racial housing practices. The level of analysis achieved is fairly limited, and unfortunately the writing is often wooden and contains expressions such as "this study hopes" (p. 20) which one has to keep one's students from repeating.

DUANE LOCKARD

Princeton University

Political Participation: How and Why Do People Get Involved in Politics? 2nd edition. By Lester W. Milbrath and M. L. Goel. (Chicago: Rand McNally, 1977. Pp. xi + 223. $4.95, paper.)

The purpose of this book is to summarize extant knowledge concerning citizen participation, which the authors define as "those actions by private citizens which seek to influence or to support government and politics" (p. 2). This revised edition differs from Milbrath's 1965 version, however, by introducing substantially more comparative material, by examining different modes of participation (communicating, protesting, contacting) in addition to electoral activity, and by including research published through 1975. The research summaries and extensive bibliography should be valuable to students of political behavior, but the book is marred by theoretical problems and by some glaring omissions.

Chapters 3 and 4, on the personal and social basis of citizen participation, are generally well done. These cover established material on political efficacy, socioeconomic status, race, age, party, and group membership. The discussion of the complex concept of alienation (pp. 61–74) is particularly good. New to this edition are extensive sections on protest activity and on the correlates of black political participation. The authors stress the greater acceptability of protest and nonconventional participation in the black subculture and draw extensively on data from their 1966–68 survey in Buffalo, New York to profile the sociological and psychological characteristics of protestors.

Other parts of the book, however, show substantial weaknesses. The emphasis throughout is on "correlates" and "predispositions," rather than on a unified theory examining the causes and the consequences of different modes of political participation. The authors present a complex "map" of the correlates of participation in their introductory chapter, and accompany this with a lengthy discussion concerning stimuli, learning, reinforcement, and selective perception. But this social-psychological perspective is almost totally lacking in political content. People do of course vary in their "predispositions" to participate, yet growing evidence suggests that decisions concerning whether or how to participate depend on a specific political context: the importance of issues, the alternatives offered in an election, appeals of specific candidates, and strategic considerations concerning the potential costs and benefits of various modes of political activity. Thus turnout varies considerably across elections, and campaign activists or contributors in one election are not necessarily active in the next campaign.

What is missing here is any conception of purposive individuals making instrumental decisions in a particular political situation. Milbrath and Goel appear to adopt (without saying so) a social-psychological or field-theory approach; they do not even mention the perspective of rational-choice models or positive theory. Their otherwise extensive bibliography contains no references to Downs, Riker, Ordeshook, Goldberg, Ferejohn, Fiorina, or anyone else prominently linked to that school. While one may criticize the assumptions, methods, or models of the positive theorists, that perspective would seem to merit at least a summary in a volume purporting to review the literature. The authors do suggest that closeness of the vote or perceived importance of alternatives affect participation (pp. 137–41), and mention stimuli and information costs briefly in chapter 2. But they fail to consider the broader theoretical implications of these components of rational models.

The historical dimension of participation is largely ignored. No mention is made of Burnham's, Rusk's, or Pomper's analysis of voting trends. The authors note (chapter 5) that legal and structural constraints may influence participation rates. But these have been eased since 1960, so why has U.S. voting turnout declined overall, while it has increased for women and blacks? Some discussion of partisan realignment and of secular and historical trends in political activity should have been included. Also, Milbrath and Goel include very little on sex differences in participation; they make almost no reference to the growing literature on the impact of women's issues, candidates, employment status, sex roles, or group identification on increasing political activity by women.

A further problem is the lack of attention to the impact of participation. In their concluding chapter, the authors do consider alternative perspectives concerning the optimal rate of participation in democratic politics and democratic theory. But, except for a reference to Verba and Nie's work on "concurrence," they ignore the payoffs for participation, either for individuals (patronage, ideological goals, social and prestige factors) or in terms of policy. As they mention (p. 10), more research has been done on participatory inputs than on what they term "outtakes." The literature on community power, school bond referenda, and comparative state politics, however, does include a wealth of hypotheses and some data on policy consequences of political activism. Milbrath and Goel might have cited work by William Keech on the impact of black suffrage, or by Fry and Winters concerning the political correlates of redistributive state policies.

It is not easy to make a summary of the literature interesting. A tighter introduction, more politically relevant examples, greater attention to theory, and a more critical approach to existing work would all help. As it is, *Political Participation* makes dull reading, especially for undergraduates. Despite its considerable limitations, the book does provide nevertheless a useful summary for graduate students and professionals of some of the voluminous material on political participation.

SUSAN B. HANSEN

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The Rural Elite in an Indian State: A Case Study of Rajasthan. By Iqbal Narain, K. C. Pande and Mohan Lal Sharma. (Columbia, Missouri: South Asia Books, 1976. Pp. 256. $12.50.)

This study is an attempt at identifying the emergence of a new rural elite as a result of the introduction of a network of democratic institutions in the north Indian state of Rajasthan. In the late 1950s, Jawaharlal Nehru and his associates felt that social change in India would not be brought about unless the average individual, especially in rural areas, was involved in it by means of a democratic process. Consequently, democratic institutions were installed at the village, subdistrict, and district level and the entire development process was routed through them. These institutions have been in operation for more than 15 years and the question is whether they have succeeded in stimulating a new leadership which may be committed to the ideals of participation and social change in general. The authors of this valuable study addressed themselves to the problem of empirically identifying the characteristics of the new elite and the extent of its commitment to the norms of democracy and development. They sought to find their answers by means of painstaking field work in five districts of Rajasthan with different systems of land tenure, rates of economic development, and party control.

Their findings suggest that social status and economic base—rather than education, public-spiritedness or political skill—continue to be the principal source of elite influence. This is further reinforced by the attempt on the part of the new elite to use its power to maintain things as they are rather than to change them. Towards the maintenance of such a status quo the new elite also received encouragement and help from local and national politicians. The latter were merely out to please anyone who ensured electoral support for them. Then there was the rigid attitude of the administrative officials who were adverse to sharing their powers with elected representatives with little education and limited administrative experience. But they had one thing in common: both were predisposed to blocking rather than facilitating the implementation of public policy involving change. Together they made a formidable team against change.

The authors conclude that in its source of strength and performance, the new rural elite in India is no different from the traditional elite. While the democratic process did strengthen the hands of the average citizen, this was offset by some measure of economic development in the last few years which largely favored those who were already economically strong and politically powerful.

The book was not carefully edited before publication, a serious shortcoming. Moreover, its theoretical argument lacks clarity. Nevertheless, it makes a valuable contribution to the study of rural politics in India.

A. H. SOMJEE

Simon Fraser University

The Myth of Marginality: Urban Poverty and Politics in Rio de Janeiro. By Janice E. Perlman. (Berkeley, Los Angeles, London: University of California Press, 1976. Pp. xxi + 341. $14.95.)

Governments of both rich and poor countries have been guilty of underestimating and underutilizing the human resources available to them. In the case of developed nations, the costs are often great, but for developing countries, the losses are even more severe. In *The Myth of Marginality,* Janice E. Perlman, an Assistant Professor of City and Regional Planning at the University of California, Berkeley, documents how an extremely negative stereotype of the nature and behavior of migrants to urban areas in Brazil produced government policies that were unnecessarily expensive, failed to take advantage of the potential contribution of the migrants, and ultimately served neither the national interest nor the interests of the migrants.

In the first section of her book, Perlman outlines the major components of the so-called "myth of marginality." According to the myth, migrants to urban areas are uprooted individuals who protect themselves through allegiance to traditional values. They are parasites on the city as well as non-participants in the political system. They constitute a "seething, frustrated mass" suffering from social disorganization and their main goal is to earn enough money to be able to return to their rural places of origin (p. 129).

Perlman then derives a number of testable propositions from the stereotype of the migrant and tests them in three types of squatter settlements in Greater Metropolitan Rio de Janeiro. On the basis of interviews with 250 people in each community, she concludes that the "myth of marginality" has very little basis in reality. In fact, her data show that the migrants "have the aspirations of the bourgeoisie, the perseverance of pioneers, and the values of patriots" (p. 243). Although this contrast between the negative stereotype and the empirical data may be slightly overstated, Perlman's findings parallel those described by Wayne A. Cornelius in his *Politics and the Migrant Poor in Mexico City.*

Given the attitudes and character of the migrants studied, Perlman argues that the best kind of public policy would be one that would legalize squatter settlements and provide aid and support for self-help projects by means of which the migrants could upgrade their homes and communities. Unfortunately, this has not been the approach of Brazil's military government. Acting on the basis of the "myth of marginality," which is congruent with the interests of land speculators and developers, the government has bulldozed the *favelas* and relocated their inhabitants in public-housing projects. The result has been increased hardship for the urban poor and the creation of "the marginalized population that [*favela* removal] was designed to eliminate" (p. 195).

Perlman has written an excellent and highly readable book which together with the Cornelius book should lay to rest the "myth of marginality." The question that now must be answered, however, is what about the second generation? In *Political Order in Changing Societies,* Samuel Huntington hypothesized that the migrants' children would constitute a discontented, frustrated and potentially rebellious group. Both the Perlman and Cornelius books, although they do not focus specifically on the next generation, do provide some support for the Huntington hypothesis. Studies are now needed to discover whether the "myth of marginality," which has been adequately refuted with regard to the nigrants, may prove to be less mythic when applied to the migrants' children.

SUSAN KAUFMAN PURCELL

University of California, Los Angeles

The Military and Politics in Modern Times: On Professionals, Praetorians, and Revolutionary Soldiers. By Amos Perlmutter. (New Haven: Yale University Press, 1977. Pp. xix + 335. $15.00.)

For more than 20 years, the relationship between "professionalism" and the political orientation of the armed forces has been a subject of intense debate. The debate started with an assertion by Samuel P. Huntington, to the effect that the armed forces could be rendered "politically sterile and neutral" by recognition of "autonomous military professionalism." As country after country—particularly in the Third World, but also among NATO members—succumbed to coups d'état, this assertion became more and more qualified. Perlmutter, in this important comparative contribution, has given the coup de grâce to Huntington's assertion.

Perlmutter's key interest lies in corporatism. Contemporary armed forces are highly bureaucratized. It is the propensity of military professionals, Perlmutter asserts, to intervene in politics and policy formation as a consequence of their corporate and bureaucratic roles and orientations, in the absence of high degrees of political institutionalization. Such a conclusion is not unusual; it has been conveyed by Finer, Abrahamsson, and many others. Power does not simply grow out of the barrel of a gun; it emerges from the organization that controls the fire power.

Any study of civil-military relations must give close attention to the conditions under which civilian rule is supported by military control. Perlmutter indicates that collapse of executive power is a "pre-condition," and the absence of a strong, cohesive middle class a "contributing" condition. His list of "sufficient explanations" (pp. 100–02) will bear close study by students of civil-military relations. Yet they will find points of controversy. Some are definitional. For example, Perlmutter asserts that military coups are organized either by a loose coalition of military political activists and their allies, or by a tight conspiracy composed of a few officers. What is excluded? A military group comes into power "when the military is the most cohesive and *politically* the best organized group at a given time in a given political system" (p. 100). In fact, military intervention in politics often reflects and results from severe problems of cohesion within the armed forces. This cannot be included within Perlmutter's list. Further, the political organization of the armed forces is an attribute difficult to sense in advance of intervention. The vagaries of personality receive little attention. As Decalo has demonstrated (*Coups and Military Rule in Africa*), the significance of individual decision and action becomes significant.

To some extent, these gaps may be due to delays in publication and the recent outpouring of studies focused on the political role of the armed forces. Perlmutter's manuscript seems to have been substantially completed by 1972, judging by dates in the footnotes. Several manuscripts to which he refers, such as those by Luckham or Thompson, have since been published, but references have not been revised.

Perlmutter distinguishes among three types of soldiers: professional, praetorian, and revolutionary. Each type is itself subdivided. Professional soldiers are examined historically in Prussia and Germany, France, Japan, and the Soviet Union. Each of these cases, according to Perlmutter, demonstrates that instability "invites" the military to intervene (p. 85). Praetorian soldiers characterize the Third World. Perlmutter examines them in the Middle East, Africa, and Latin America, differentiating between historic and modern praetorianism. The praetorian army, he asserts (p. 98), tends to "replace weak and unstable political groups and regimes." Post-hoc explanation? Can one determine weakness and instability prior to intervention? The third type of soldier, the professional revolutionary, is subdivided between revolutionaries and routinized revolutionaries. The former included the People's Liberation Army of China, the latter the Zahal of Israel.

Readers familiar with Perlmutter's previous works will find much here that is familiar. He has further developed his distinction between arbitrator and ruler regimes. He draws primarily on the Middle East–and particularly on Egypt, Iraq, and Syria–for his evidence. He goes into exhaustive–and perhaps exhausting–detail about Israel. Updating his 1969 book, he views Israel as a "routinized revolutionary army." The material on Africa and Latin America is drawn from secondary sources, with generalizations designed to show contrasting levels of corporate professionalism.

Perlmutter's book is wide-ranging, challenging, generally well documented, and usually consistent in its arguments. *The Military and Politics in Modern Times* chronicles an undoubted phenomenon: The increasing tendency of the armed forces to become the most prominent political actors. I applaud his conclusion that "political conditions dictate the nature of civil-military relations," that "the military is motivated to intervene only when its corporate or bureaucratic roles seem threatened..." (p. 281). Some readers will note a striking contrast in detail; the section on Israel is overwhelmingly complicated, the pages dealing with tropical Africa peppered with few examples. Perlmutter's prose is labored and repetitive on occasion. Dichotomies are more clearly cut in theory than in actuality, yet his thoughts are provocative and his conclusions well substantiated. Perlmutter has done much more than examine military intervention; he has examined patterns of military rule. Surely this is an area to which increased attention must be given. Little is said, by contrast, about the nature of behind-the-scenes military influence; the means (if any!) of military disengagement from politics are discussed only elliptically. These questions should not, however, fault what is a useful and interesting, if hard to read, comparative study.

CLAUDE E. WELCH, JR.

State University of New York, Buffalo

The United States and the Andean Republics: Peru, Bolivia, and Ecuador. By Fredrick B. Pike. (Cambridge, Mass.: Harvard University Press, 1977. Pp. 493. $25.00.)

Fredrick Pike's latest book is less a study of United States policy toward Peru, Bolivia, and Ecuador than an historical analysis of the Andean republics themselves. Less than 40 of the text's 381 pages relate specifically to the relations of United States public and private sectors with these three countries. In this respect it is similar to another recent book in the same American Foreign Policy Library series, Arthur P. Whitaker's *The United States and the Southern Cone: Argentina, Chile, and Uruguay* (1976). Both are useful, however, not primarily for what they have to say about United States-Latin American relations but rather for their articulation of the distinctive patterns of social, political, economic, and cultural development of various South American nations.

For Pike, Professor of History at Notre Dame, these distinctive development patterns in the Andean republics are rooted in the traditions imposed first by the Inca Empire and later by the Spanish Crown and the Spanish Church. Pike is one of the small but prolific group of Latin Americanists who characterize these traditions as corporativist and who see in them the key to understanding why Spanish and Portuguese America have evolved differently from other regions of the world.

The corporativist traditions are linked at the structural and the individual levels in the analytical construct which Pike derives from the historical record. Corporatism thus encompasses both the hierarchical and functionally organized social, political, and economic institutions and a world view of the individual based on dependency, paternalism, and community. This corporativist mode was particularly well institutionalized in Peru, Bolivia, and Ecuador for a number of reasons, among them the importance Spain attached to this part of the empire due to its large mineral and labor resources, the length of the colonial period (almost 300 years), and the close congruence between Spanish institutions and perceptions and those of the preexisting Inca Empire.

To this point, Pike's analysis more or less parallels that of the "Cultural Corporativist" school articulated by the late Kalman Silvert and elaborated upon by Howard Wiarda, Richard Morse, Lawrence Graham, Ronald Newton, and others. Where Pike tries to break new ground is in his attempt to explain the retention of the corporativist patterns through the Republican period and their resurgence in the 1960s under nationalistic military regions. To do this he mixes institutional and individual-level analysis in an historical context to generate a number of intriguing hypotheses:

(1) That the constraints of corporativism did not apply to the same degree to the elites who controlled the uppermost rungs of the functionally divided hierarchies. They were free to experiment with liberalism—with its stress on laissez-faire, individualism, and a state serving the private sector, and they did so to the degree they could benefit from it.

(2) That the extraction of resources by the Crown continued in the Republican period by elites and foreign entrepreneurs. Hence, the dependent and clientelistic world view of the masses continued to be constrained not only by tradition and culture but also by extremely limited economic resources.

(3) That the elites prospered within the agricultural, financial, and commercial sectors, but did not attempt to carry out large-scale industrialization. Hence opportunities to expand the liberal model to lower strata of the population was quite limited. When pressures grew after the turn of the twentieth century to open up the system to newly mobilized elements of the population, the elites responded by using the state to reassert prior modes of patron-client relationships, in effect abandoning liberalism and renewing the corporativist system.

(4) That intellectual and theological currents of the 1880–1930 period, most notably *arielismo, hispanismo, indigenismo, Rerum Novarum,* and Catholic action, reinforced the disillusionment of elites with liberalism and helped move leadership to reassert corporativist patterns in both rhetoric and law.

(5) That the rapid expansion of U.S. private interests in the hemisphere between 1900 and 1930 brought many Latin elites into close proximity with the reality of the imported liberal model for the first time. Many became disillusioned with the experience, particularly with what was perceived as crass materialism and venality. Others saw in the increased United States presence a convenient political whipping boy. This reinforced the move toward reestablishment of inherently Latin traditions and practices.

(6) That the rise of military corporativism in the last two decades reflects the continued rise of nationalism, concurrence with new Church social-action concerns, and, above all, the failure of civilian elites to carry out modernization within Latin traditions without raising the spectre of social breakdown.

To give the reader a comparative context within which to examine these hypotheses, Pike provides considerable historical detail. Nevertheless, the elaboration of supporting evidence is strangely lacking. One does find considerable information on the Church and on leading intellectuals. One expects to find a systematic examination of the statements of leadership, a review of major legislation and constitutions, and the perceptions and actions of the mining, agricultural, and commercial organizations of the elites, but there is none. Even public sector policies and initiatives are covered only in part. Without this systematic elaboration of evidence the hypotheses generated remain by and large unsubstantiated assertions.

Nevertheless, Pike has provided the reader with much food for thought and has indicated several directions for research for those interested in the possible explanatory value of the corporativist model. However, *The United States and the Andean Republics* is not the definitive work one wishes it might have been in spite of the impressive talents of its author.

DAVID SCOTT PALMER

Foreign Service Institute

The Yugoslav Experiment 1948–1974. By Dennison Rusinow. (Berkeley and Los Angeles: University of California Press, for the Royal Institute of International Affairs, London, 1977. Pp. xviii + 410. $16.50.)

The battle between the descriptive detail of narrative history and the explanatory expectations of postwar social scientists has not been fought on the fields of Yugoslavia. Where contemporary political history has been anything but dull, the shelf of its chroniclers—and thus of knowledge about "context" so necessary to explanation—is practically empty. Dennison Rusinow, who since 1963 has provided the most lucid and reliable analyses of Yugoslav events to readers of the occasional reports of the American Universities Field Staff, has finally filled the void.

Rusinow was clearly the person to perform this task: trained in Adriatic and Central European history, long resident in Yugoslavia, he has a journalist's sense of the noteworthy, a treasury of contacts, and an uncanny ability to find gold among the tin of officialese.

The book begins with the assumption of control by the Communist party and thus with the "birth of a new Yugoslavia." It ends when, in Rusinow's view, the party has regained control after the "crisis in Croatia" of 1969–1971, and this remarkable regime enters a new stage in its life cycle. In form, if not in style, it tells a story—with a prelude, climax, and denouement, with frequent asides to lay the scene and describe the mood, and with careful attention to the development of characters.

This story, of the "odyssey" of Yugoslav Communist leaders in search of a socialist solution to the problems of the former regime (modernization, independence from foreign powers, and ethnic conflict), turns rapidly into a drama of near-collapse under the tension of ethnic nationalisms. Throughout the tale—of elite controversy and competition, of the evasion of self-imposed political and ideological dilemmas by compromise, by maintaining incompatible decisions, or by "papering over" signs of strain (p. 111), of political polarization by economic difficulties (which act "like a chemical precipitant" [p. 112] on conditions of dissensus within the elite), of the ensuing struggles whose outcome depends as much on the political skill of the particular actors and the current rules of the game as on the merits of the issues for Yugoslav development—runs the thread of Rusinow's argument: the changing role of the party. Stages of postwar history are defined by the dominant political camp and its view of the role of the party. Each new stage follows a crisis when the resistance of one faction is overcome by the skill of another in pursuing its chosen strategy and when Tito makes personnel and organizational changes to "save the party" (issuing the same indictment of "faulty organization" and "inappropriate mentality" [p. 191] for each set of losers).

The rule of the "conservatives," for example, and their "centralized party oligarchy" (p. 192) and tactics of cadre control begins to give way as early as the debates of 1950–52 (occasioned by the Cominform Resolution crisis and sanctioned by the Sixth Party Congress) over the role of the party, now, in a socialist democracy. The anti-centralist "liberals" with their pluralistic polyarchy and Constitution-writing ascend to power only after changes in the party have weakened internal discipline, caused "confusion in the ranks," and made consensus difficult. The change in rules of the game and in rulers is marked, as usual, by a party congress (the eighth), a new constitution (1963), and an important purge (of Ranković and the secret police). The post-1966 system lasts until weaknesses of the now ill-defined party lead to a scramble for control (p. 192) and to paralysis of decision making at the center. Tito's *coup de main* to save the party (this time a conservative one) forces a reassessment, sanctioned by a party congress (the tenth) and a new constitution (1974), of the role of the party. The post-1972 return to a Leninist role for the party and a harder line toward political freedoms and intellectual dissent which Westerners have found hard to stomach is, nonetheless, in other respects not a return to the pre-1964 system: not only are the Tories busy "dishing the Whigs" but also by Rusinow's analysis the last of the story has not been heard.

The limits of Rusinow's method are obvious. We learn much about the palace and little about the people. References to popular attitudes seem to be more often to lower-party cadre than to ordinary Yugoslavs. But for those readers who will miss sociological analysis, there will be as many grateful for the attention to economic decisions and the refusal to discuss polity without economy. Discussions about political and ideological alternatives are limited for the most part to what is "in the air," and since political struggles in Belgrade and Zagreb still dominate the federation, one hears little from the "periphery." The decision to keep analysis implicit leaves much raw material for others to mold. The value of chronological organization in illustrating the gradual accumulation of irreversible decisions and mistakes and the importance of timing and context is reduced by the insidious persuasiveness of genetic explanation (e.g., in the nationalist developments) which is not altogether justified by the information Rusinow provides. With the author's purpose clearly in mind, however, the reader is treated to remarkable pictures of Tito and Bakarić as politicians, to trade union activities independent of the party, to a Yugoslav Watergate, and to fascinating descriptions of the atmosphere among top decision makers in 1948–49, of the resistance to the reform and the purge of Ranković, and of the tactical errors of the liberals.

It is a mark of the quality of this book that it cannot be summarized, nor the subtlety of its interpretations portrayed. It provides the context in which all else about postwar Yugoslavia should be read; it is that book I have been waiting to recommend. Though not everyone will agree with its emphases, it should form the basis for a long-needed exchange on the dynamics of the current Yugoslav system. For students confronted with the nation's tighter climate, it can be a manual on how to extract the most from official sources. That it requires a great deal of chewing and digesting only makes one deplore more the publisher's decision to economize by placing footnotes at the end.

SUSAN L. WOODWARD

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Enoch Powell and the Powellites. By Douglas E. Schoen. (New York: St. Martin's Press, 1977. Pp. xviii + 317. $18.95.)

Enoch Powell is an improbable figure to have become the most controversial populist politician in Britain for 40 years. Personally remote, a scholarly figure studiously avoiding creating a personal political organization, Powell nonetheless frequently outdistanced the leaders of the two principal parties as the preferred candidate for the premiership in the polls. Douglas Schoen offers a political biography of Powell and an examination of his followers from 1969–1976.

Schoen traces Powell's career from relative obscurity on the Conservative opposition front bench to his role as tribune of the people with dire warnings of racial conflict if nonwhite immigration into Britain was not stemmed, indeed reversed, by "repatriation." Schoen contends that Powell has not remained a single-issue politician but has offered a broad program of substantive change. Long a monetarist, he attacked the government of his fellow Conservative Edward Heath for adopting centrist economic policies. He vigorously opposed Britain's EEC membership as a violation of nationhood and parliamentary sovereignty. Such positions increased his isolation from the Conservative leadership and ultimately led to his remarkable endorsement of Labour in the 1974 elections. More recently, Powell has been returned to Parliament from the Ulster seat of South Down—a move both literally and figuratively to the periphery of British politics. Powell contends that Ulster is an integral part of the unitary British state under the Westminster Parliament's direct authority. This position holds little interest for those in England and antagonizes Powell's Ulster Unionist colleagues who favor a devolutionary political solution.

Few would debate Schoen's contention that Powell has been influential on Labour and the Conservatives in further tightening restrictions on immigration. More debatable is that renewed emphasis by Conservatives on the return to a capitalist economy is largely an accomplishment of Powell's sustained criticism of the Heath Government. Certainly dissatisfaction with Heath's policies was shared by a significant number of Conservative MPs for a number of years. Schoen believes Powell to have had a negligible effect on public reaction to British entry into Europe and to Ulster.

A point of interest to American observers is that Powell's challenge to his party's leadership and the elite consensus on immigration made him more popular in Britain than George Wallace became in the United States. Yet Wallace became a major presidential contender but Powell was never considered for the party leadership after 1969. Schoen quite accurately points out that British political institutions do not lend themselves to the ambitions of a protest politician. Few would argue that British politics are as permeable as American, yet the enigma of Powell which Schoen does not explore is his failure to build a faction within the Conservative party or launch a third-party movement. Unlike Joseph Chamberlain—that other midlands politician of 70 years ago who successfully abandoned the Liberals by taking his followers into the Conservative party—Powell has not realized organizational capital from his successful electoral interventions in 1970 and 1974. Perhaps there is a clue in Powell's chiliastic rhetoric. He may see himself as a Churchill waiting for an internal second world war in order to be called to power. We can only speculate and regret that Schoen was unable to secure an interview with Powell to explore this question.

The second part of the book critically examines sociopsychological models of mass political movements. Schoen concludes from his analysis of the surveys available to him that such concepts as status insecurity and relative deprivation do not distinguish Powellites from supporters of the conventional political establishments. Schoen is persuasive in suggesting that Powell's following should not be explained away as sociologically aberrant. Butler's and Stokes' studies (1975) reveal both the breadth and depth of the British reaction to immigration and race relations. Where Schoen may be faulted is that in his investigation of the Powellites he relies on both market research surveys and the Butler and Stokes studies. As a consequence, his definition of a Powellite varies not only over time but between surveys. Unfortunately for the researcher, Britons never have had the opportunity to vote directly for Powell or to work in any sort of Powellite organization, so harder evidence of a Powellite commitment is impossible to come by. Schoen contributes to this problem by offering a generous definition of what it is to be a Powellite. He tricotomizes the responses to the Butler and Stokes thermometer evaluation question of Powell. Respondents are "Powellites" if they have the beginnings of a positive feeling towards Powell. It would have been more useful to distinguish several groupings of Powell supporters based upon intensity of support.

Schoen's study is a useful and well-written account of Powell and the massive popular response to him particularly on the race question, but more must be done before we can fully comprehend the relationship between Powell, the race issue and British party politics.

JOHN G. FRANCIS

University of Utah

Education, Class and Nation: The Experiences of Chile and Venezuela. By Kalman H. Silvert and Leonard Reissman. (New York: Elsevier, 1976. Pp. ix + 242. $13.50.)

This is a rich and finely crafted comparative study of the relationship between education, class, and national development, by the foremost U.S. scholar of Latin American politics and his long-time intellectual associate, a sociologist. Theory, survey data, and history are interwoven to produce critically important conclusions about the contemporary crises of Chile, Venezuela, and Latin America generally. The book reflects the authors' ten-year effort with this study of 9000 primary and secondary students, their teachers and parents, and university students (which samples were limited in size and utility by the political climate during data-collection periods). During this decade (1965–1975), the core issues of the study were under intense debate and at the crux of politics in the two countries as well as in the comparative political sociology/development sectors of U.S. academia. The authors struggled vigorously with these issues and present their commitments clearly: to rationality and free political inquiry, to increased societal self-knowledge, for the augmentation of available choice. Their commitment to equitable democracy is similarly clear.

The book focuses on the relationship of education to preparation for citizens' participation in secular, relativistic politics. The key concept is modernism-traditionalism (the recent troubled intellectual history of which is forthrightly confronted) distinguished by rationality/ritualism, relativism/absolutism, and orientation toward change and capacity to effect it. The study was tightly designed. Indicators of modern attitudes about behavioral motivation, standards for behavior, and reactions to change were devised for five institutional areas: family, religion, economy, education, polity. Primary students were found to be most traditional, followed by parents; secondary students reflecting most modernity. Chileans were consistently more modern than Venezuelans, group for group, institution for institution.

An ideal typology of world views was constructed by distinguishing people on self-autonomy, autonomy for others, and institutional autonomy. The resulting empirical types were elaborated and given extraordinary depth by the authors' understanding of Latin American history, so that there is a strong ring of authenticity to their discussion of "corporativist technocrats," "corporate pluralists," and "autonomomists," in real political struggles down through time. No background characteristic (class, sex, etc.) had "even remotely as much influence on type of world view as has educational experience, whose effect is overwhelming" (p. 99).

The concluding chapter contains a solemn discussion of education and crucial political choices confronting Latin Americans, e.g., the tension between equality and privilege resolved in horrendous repression by the 1973 Chilean military coup. The authors note that authoritarian governments assume the task of stripping inquiry and relativism from education to produce children-subjects. They conclude: "In developing capitalistic societies, the principal brake to continued development has been a stress between the opposing pulls of class and nation. The intellectuality promoted by prolonged participation in formal education, however, tends to lead educated persons to value universal national over particular class interests. Therefore, while education as an institution makes possible partially democratic as well as dictatorial national integration, through time it is inherently in contradiction with class-supportive authoritarianism" (p. 183).

My major criticism concerns the basic traditional/modern dichotomy, which reflects a bias toward free-floating individualism. This is easy to accept when contrasted to "traditional" rigid social constraints, but harder to accept in relation to the need for a community that promotes individual development by providing equitable social conditions. For example, regarding the institutional autonomy dimension of the world-view typology, two of six items ask: "Ought the price of essential goods be fixed by the government?" "Is the principal function of the school to prepare individuals to earn a living?" "No" to the first and "Yes" to the second are classified as "autonomous" modern responses. The argument for an autonomous economic institution as "modern" seems to lack theoretical as opposed to ideological justification. This is important because it goes to the heart of the issue between class and nation about which Silvert and Reissman are otherwise so penetrating. A similar bias is apparent in the treatment of the school item, since an affirmative response ignores the range of crucial pro-development functions schooling might perform ahead of or coequal with the individual's earning a living. Regarding the according of autonomy to others, an item asks, "Which is the better criterion to judge a man, by his contribution to society, or by his talents and capacities?" The latter is the "modern" response. While many would share a concern about the deadening function of social constraints, whether informal or heavily sanctioned by governments, the costs of capitalism's emphasis on the individual, abstracted from the collective condition and community welfare cannot be dismissed. However, the scoring of the data is such that these biases alone would not result in the assignment of a respondent to "traditional" categories. So the cost comes at the level of elucidating the critical issues between "modernists" of capitalist vs. socialist or communitarian persuasion.

A final concern from an environmental perspective is the authors' rejection of "organicism" in favor of relativism. "The traditional person by this construction will tend to see every act as immediately related to the universal, thus affecting everything at the same time. . . . The modern person does not assume universal consequences from his day-to-day actions, but tends to see historical time as absorbing the shocks of the effects of immediate activity" (p. 19). By the crucial criterion that everything is connected to everything else, beware the modern person!

This is a profound and rich work. Sorrowfully, as one who learned so much from Kal, I must signal it a legacy. Reissman and then Silvert died shortly after its completion.

DANIEL GOLDRICH

University of Oregon

Must Canada Fail? Edited by Richard Simeon. (Montreal: McGill-Queen's University Press, 1977. Pp. vi + 307. $13.00, cloth; $5.95, paper.)

A collection of essays written primarily by scholars who teach at Queen's University in Kingston, Ontario, *Must Canada Fail?* is aimed at an English Canadian audience. The book deals with the potential threat an independent Quebec poses for Canadian federalism and the political dilemma of Quebec's future in Confederation. Readers are assumed to have familiarity with the political institutions, the economic structure, and the cultural context of Canada. The authors do not aspire to a rigorous treatment of the subject matter, but rely instead on their own intuition. The danger of such an approach is that selective perception takes over and conclusions risk being merely the reflections of each author's own well-intentioned predilections and biases. Only a few of the articles make reference to the now abundant Canadian survey data on the topic.

The first part of the book presents background material on the Quebec-Canada conflict. The second part deals with the different Canadian regional perspectives. The last part of the book, "The Longer Run," and the most interesting by far, considers possible scenarios for the future.

All of the authors represented are avowed federalists. Yet, as the book demonstrates, a variety of political postures exist within this framework. Some of the articles manifest a certain degree of sensitivity to Quebec's present position on independence, and sovereignty-association. Others are so narrow and prejudiced as to border on racism.

The voice of Quebec francophones themselves is completely missing from the book. None of the articles provides an understanding of the aspirations of the Quebec people, of their economic and cultural oppression in North America, or of the logical conclusions of their desire for increased independence and self-autonomy. *Must Canada Fail?* provides little insight about the internal functioning of the Parti Quebecois, though a number of excellent accounts are presently available in French concerning these matters.

After reading this book one must conclude that the November 15, 1976 election of the Parti Quebecois took English Canada by surprise, a surprise from which it has not as yet recovered. A number of articles repeat the following themes: (1) the PQ is a strong, competent, and dynamic government; (2) its desire for independence must be taken seriously; (3) the Canadian people as a whole will not accept keeping Quebec in the confederation by violence, and finally (4) cool-headedness, persuasion, accommodation and compromise are the keys to a "rational" and "gentlemanly" solution to the problem of Canadian unity. The purpose of the book itself is to search for options acceptable to the English Canadian community, to explore the possibilities available. Few of the contributors are optimistic about the future. A resigned pessimism constitutes the general tone of most of the essays.

The book shows signs of having been hastily written and quickly put together. Certain articles are repetitive and poorly documented. Events in the last six months have already passed this book by, the inevitable result of a highly fluid political situation. It is significant that some of the recent events which have taken place subsequent to the publication of the book were not anticipated by the authors. One striking example is Pierre Elliot Trudeau's recent threat to send in the army if Quebec tries to separate "illegally." Another is the recent revelation that the federal government has for several years conducted a major surveillance operation on the Parti Quebecois through the Royal Canadian Mounted Police.

The authors of the articles in this book do not go much beyond the parameters already put forth by federalist politicians in the Canada-Quebec debate. If English Canada has little more than this to offer, then the question posed by the title of the book risks becoming a self-fulfilling prophecy.

PAULINE VAILLANCOURT

University of Quebec, Montreal

Teoría, Acción Social y Desarrollo en América Latina. By Aldo E. Solari, Rolando Franco and Joel Jutkowitz. (México: Siglo Veintiuno Editores; Textos del Instituto Latinoamericano de Planificación Económica y Social, 1976. Pp. 637. Price not listed.)

Although this book, projected by the Latin American Institute for Social and Economic Planning (ILPES), has a somewhat misleading title, its real purpose is to offer an overview of contemporary Latin American sociology. It is not a history, as the authors hasten to make clear, but a "systematization" of prevalent trends, findings and failings, without neglecting polemics. Since political science in the region has not yet achieved full autonomy, many of the questions examined here would be the object of our discipline in different latitudes.

According to the authors, Latin American sociologists since 1940 can be classified as followers of two general "orientations": scientific and critical. Initially the former appears to be primarily represented by José Medina Echavarría, Florestán Fernández, and Gino Germani. But later, and for different reasons, Medina and Fernández are dropped, leaving the scientific "orientation" headed by Germani alone. There is not such specificity on the followers of the critical trend. But after skimming the footnotes, one may conclude that among the most representative are Orlando Fals Borda, Fernando Henrique Cardoso, and Pablo González Casanova.

Part 1, 22 chapters, tries to characterize both "orientations" and point out the differences between them. Three other successive parts discuss the problem areas to which the general conceptions have been applied: The Agents of Change and Conservatism in Latin America (Pt. 2): Structural Conditions, the Insertion in the International System, and Domestic Relations (Pt. 3); and Values, the Polity, and Planning (Pt. 4). Reasons for adopting such organization are given in one short page (p. 201). They are not only short but questionable. With the exception of Part 2, it is difficult to perceive the internal congruity between the topics covered by Parts 3 and 4. In part 1 the agents of change are identified as social classes or strata. But under the heading "upper classes or strata," after the oligarchy and the entrepreneurs, this section discusses the military, religious elites, bureaucrats and technicians. Surprisingly enough, there is no special treatment of the intelligentsia, although its role as a top influential agent of change in the region hardly can be overlooked. Throughout the book only occasionally is the distinction announced in part 1 elaborated.

To review briefly a book of this size (637 pages) is frustrating. It is even worse when, as in the present case, the work lacks of any index or bibliography, and the epilogue, which the introduction promises will offer a balance "between achievements and frustrations," only adds new "reflections." Undoubtedly the authors have performed a strenuous job and deserve due recognition by scholars interested in the area. The length of the book, however, raises one legitimate question. As it is so eloquently argued here once more, Latin American Sociology has made significant contributions to our knowledge. But does it really deserve such a massive work, precisely at a time when we are unrelentingly urged to save energy? Would it not have been more useful, and justifiable, to have instead a handy text, covering the essentials, and including an index as well as an annotated bibliography?

Even if readers are not so time-conscious as I am, they may advance another important objection: there is a total lack of reference to empirical works in this book. The authors explain such omission by stating that what they call theory is "much more important than empirical data" (p. 85). This is indeed a curious argument to be made by scholars who have discerned a scientific "orientation" as one of the principal trends in Latin American sociology. In any case, although they are not overabundant, there are significant empirical contributions in the area which should not be missing in such an ambitious project as this.

Finally, in a book where even "pensadores" deserve 88 pages, it is striking that outstanding politician-intellectuals, like Victor Raúl Haya de la Torre, José Carlos Mariátegui, and Jorge Abelardo Ramos, to mention only three of them, receive totally inadequate attention, if any at all.

ANDRÉS SUÁREZ

University of Florida

The Promise of the Coming Dark Age. By L. S. Stavrianos. (San Francisco: W. H. Freeman, 1976. Pp. x + 206. $8.95.)

The double message of this compact, far-reaching and—note!—basically optimistic work (called by Joseph Church, "of all prognostications and prescriptions published in the last year or two [the only] one worth the paper it is printed on," *New York Times Book Review*, 22 Aug. 1976, p. 3) is in its paradoxical title: its accent lies on the "promise" although (or rather, it insists, *because*) our age variously parallels that of the decaying Roman Empire. The erudite author considers our civilization largely a wreckage, but exhorts us to view this at our peril, as gradual demise of structures and institutions which have become a "constricting and exploitative anachronism." The demise was then, and is now, *required* for, and already accompanied by, the sprouting of an eventually higher civilization.

While aware of fundamental differences, Stavrianos finds "Today's Dark Age" to be "remarkably similar" to Rome's, in four aspects: (1) *Economic imperialism:* then it was achieved by direct plunder, now by neocolonial modalities, especially transnational corporations. (b) *Ecological imperialism:* The Mediterranean basin has never recovered from Rome's deforestation, soil erosion and depopulation policies; today's rich nations live off poor nations' foodstuffs, energy and raw materials. (c) *Bureaucratic ossification:* the causes differ but the resulting, ever more incurable ineffectiveness of a built-in, rampant bureaucratism in the same. (d) Finally, today in the West there is a flight from reason toward vague theologisms (and, one could add, toward demonology and thanatology) reminiscent of late Rome's mysticism; and "a feeling of irresistible decline" ominously spreading among influential western intellectuals now, similar to that of, say, old Seneca and St. Cyprian then.

Stavrianos finds the rationalizations of today's pessimism as flimsy as were the quaint theological constructs which, after Christianity became part of Rome's establishment, supported as "designed by God" the very Empire that the earlier Christians had rejected. All this delayed then, as it delays now, but did not prevent then, as it does not prevent now, the great and necessary transformations. This examination of the decay-*cum*-promise aspects of our era deserves high attention, even if one were to dismiss the now-like-then analogies.

For, in any case, the author sees decay and great dangers in the stubborn adherence to certain "myths" all of which depict merely sociopolitical problems as "inherently insoluble." In particular: the fashionable trend to doom social changes, even if already made or evidently overdue, as either undesirable or impossible because of "human nature," which is posited as "singularly . . . selfish, narrow-minded, covetous, and bellicose"; the "myth" that population growth has condemned the human race to decimation by mass starvation or war; the "myth" of the uncontrollability of a gallopingly nefarious technology.

All of these, and other problems can be dealt with—although *not* through "the triad of bosses, bureaucrats and experts" of "the corporate industrial society" (p. 22) which the current technological revolution has made as obsolete, as the first Industrial Revolution made "the triad of kings, priests and landlords." This is a big assertion, but Stavrianos bases it on the inherent logic of interconnected trends; and, on the other hand, on the failures to which adherence to fallacious axioms are leading.

Thus, he sees a particularly telling sign of obsolescence in the continuing belief that "an ever-growing Gross National Product is the cure-all for social problems." It has shown its unworkability even in the U.S., where a trillion-dollar-plus GNP is accompanied by worsening societal ills. Also, our depletion of finite global resources, and our incapacity to manage, e.g., our ever-mounting waste products, disqualifies "the American consumer society [as] model for the rest of the world" (p. 58).

Yet, some Third World countries follow the idolatry of the growing GNP, by relying on foreign investors, high profits, low wages and mass unemployment, characteristically under ruthless, counterrevolutionary dictatorships. The book considers this "Brazilian solution" (Brazil being the foremost example of such industrialization) to be doomed to failure, citing, inter alia, its denunciations by the country's Catholic hierarchy (p. 174–79). The most promising model for the opposite development strategy is China, and he expects it to be increasingly followed. Its essence is not its labor-intensity but its community spirit.

Stavrianos welcomes a trend "From Aristo-Technology to Democratic Technology": mammoth industry might have begun giving way to a "centrifugal industrial technology" assisted, among other things, by new lightweight materials, and miniaturization. Similarly, capital-, energy-, and chemicals-intensive agriculture "is becoming economically, ecologically, and socially non-viable" (p. 37) even in the U.S. and, as the failure of the analogously conceived "Green Revolution" proved, all the more so for the Third World. Altogether, there the outlook is for a combination of selective modernization with indigenous community traditions, which would mean "true decolonization."

But the book goes further. Its core might be the chapter, "From Representative Democracy to Participatory Democracy" in political organization everywhere (pp. 83–135). Western-style representative but intrinsically nonparticipatory democracy is "becoming increasingly non-functional throughout the world, because it requires an ever-growing but ever less efficient and less manageable bureaucracy. . ." (pp. 84–86, 91). In the United States, for example, the system has resulted in "the gutted American countryside, the increasingly uninhabitable megalopolis, and the unprecedented lack of public confidence in traditional institutions. . ." (p. 103). Everywhere, the only way out is suitable decentralization through self-management, community activities, neighborhood associations, citizens' committees, and the like. He agrees with Sacharov that this is also compatible with, and in fact, demanded by, a socialist system (pp. 108, 111, 114). Altogether, there is coming "a new age of . . . greater self-knowledge" and "demand for self-management in all phases of life" (p. 24) including a new "workplace democracy" already experimented with in various parts of the world. Even success in guerrilla warfare (see Vietnam and Guinea-Bissau) hinged on a participatory mass-movement and simultaneous build-up of a new society (pp. 116–23). And globally, the trend of reorganization appears to be toward regional autonomy Some of Stavrianos' arguments can be questioned. But this does not diminish the urgency of his message: the plea to the West, to cease to be (as, for example, Heilbroner had already warned in 1959 in *The Future as History*) "no longer in tune with the forces that are preparing the environment of the future" because this would exclude the West from "participation in mankind's journey." The West is, and remains, so powerful that its further opposition to objectively required changes would cause enormous harm—*also* to the West.

World-wide interdependence is a historically new datum. Rome's dark age affected only part of the world. There is now no city on the Nile to which Alexandria and Washington could be transplanted from the Potomac—and, let us remember, it was in Alexandria on the Nile where Justinian's Code, perhaps ancient Rome's biggest legacy, was compiled in its evening glow. Also, alarmingly, now the *world* has fewer *decades* than *Europe* had *centuries* for the transition. Can human beings, Stavrianos asks, change their thinking and acting fast enough? He rejects despair, with two (un-Marxist, modified dialectic growth-in-the womb) arguments: (a) The present system is in such disarray, so crisis-prone and unable to reach its own goals (cognitive dissonance) as to foster an abrupt quantum jump, comparable to the Protestant Reformation or the French Revolution. (b) The existence side by side of much "global unity" (he means: interpenetration) *and* global diversity in social philosophies and institutions facilitates global interaction and cross-fertilization by "any creative achievement anywhere" (transposition of the biological law of hybrid vigor). These two arguments imply (c) that there need by no means be an ultimate capitalism-communism clash but there is under way an *"Aufhebung"* ("maintaining" in the double dialectic sense of "preserving" and "overcoming") of both.

All, then, depends on the adaptability of developed societies; and the author rightly insists that the very globality of the present age refutes the cliche of "the torch falling from Western hands." Rome's great creations survived its dark age and are still dominant all over the world; the truly constructive Western contributions will survive and, indeed, flourish in the future. But if the necessary adaptations are not made, mankind is headed, to put it very soberly, toward enormous calamities.

JOHN H. E. FRIED

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Politics in Hungary. By Peter A. Toma and Ivan Volgyes. (San Francisco: W. H. Freeman, 1977. Pp. xii + 188. $12.00.)

The authors, both knowledgable and skillful analysts of East European politics, exhibit laudable realism in their prefatorial caveats regarding the applicability of the Almond-Powell comparative politics model to the Hungarian system. One methodological difficulty that they note is the dependence of the Hungarian regime on the Soviet system, whereas the model assumes a discrete and differentiated political culture. Another problem is the model's correlation of development with greater independent capability and domestic integration, propositions that again are belied by the Hungarian case study, which shows that a command system and foreign hegemony prevail alongside significant economic reforms. The concept of conversion functions is also strained by a system where "the Party elite has tried to generate those demands it is interested in satisfying and to stifle those demands it cannot meet" (p. ix).

The result is a somewhat procrustean exercise that attempts to dissect the Hungarian political system largely in abstraction from its international environment. The first five chapters provide an overview of the system's development since 1945 and of the party's structure and major policy shifts, as well as an analysis of the patterns of political participation and policy and rule making. There follow examinations of rule adjudication, of political socialization and the role of the media, and of the social structure and the allocative functions of the system.

Although the organization imposed by the model results in some awkwardness and unnecessary repetition, the authors provide much useful information and many valuable insights. They present an authoritative picture of the system's development from a protracted mobilization period (1949–62), marked by an early totalitarian phase and interrupted by a devastating political collapse in 1956, to a postmobilization period distinguished by Kadar's alliance policy and by the introduction of the New Economic Mechanism in 1968. The unresolved and perhaps unresolvable problem is "how to achieve democratic legitimacy (a Western concept) without destroying the leading role or supremacy of the Party (a Communist concept) in Hungarian society" (p. 63).

The question of legitimacy is addressed also in a penultimate and all too brief, but delightfully evocative chapter on the political culture. When the authors estimate that the two deviant political subcultures—the "dedicated, ideologically motivated leftists" and the "strong, anti-Communist proponents of national independence"—together account for no more than five percent of the population (p. 142), they may be misled by superficial apathy as far as the latent strength of the second orientation is concerned, a strength underscored, moreover, by the pervasive nationalism noted elsewhere in the chapter. Nevertheless, the general conclusion that "the cost of the Party's successes and failures has been the creation of one of the most cynical political cultures that ever existed" (p. 159) is a compelling one. The failure of the 1956 revolution and the inescapable facts of Soviet hegemony and Communist rule have induced a widespread apathy and materialism that has been skillfully exploited by Kadar to produce a modus vivendi between party and populace resting on the pragmatic economic modernization policies of the former and the tacit political acquiescence of the latter. The resulting political stability may not represent everyone's idea of legitimacy, but it is a closer approximation than that enjoyed by the other East European regimes. Meanwhile, "the Party carefully delineates the parameters of [Hungarians'] action: no citizen may challenge the policies decided by the Party elite, and the political leadership, in turn, must elaborate policies that express enthusiastic support for the USSR vis-à-vis its position in the international political arena" (p. 154). It is only one of the many discontinuities in the system that the regime "has had no success at all in altering the negative orientation of Hungarian citizens toward alliance with the USSR" (p. 157).

Politics in Hungary is a valuable study that enriches knowledge of the Hungarian system and indicates some strengths and weaknesses of the comparative politics model. Theorists as well as specialist students of East-Central Europe will benefit from reading this work.

BENNETT KOVRIG

University of Toronto

Stalinism: Essays in Historical Interpretation. Edited by Robert C. Tucker. (New York: W.W. Norton, 1977. Pp. xx + 332. $19.95.)

The series of essays written by Wlodzimierz Brus, Katerina Clark, Stephen F. Cohen, Alexander Erlich, Leszek Kolakowski, Moshe Lewin, Robert H. McNeal, Mihailo Marković, Roy A. Medvedev, T. H. Rigby, Robert Sharlet, H. Gordon Skilling, and Robert C. Tucker addresses itself to Stalinism as a historical problem. The authors represent different disciplines, political predilections and nationalities, a circumstance which makes this volume eclectic, full of disagreement and yet (or perhaps therefore) richly rewarding. The authors are united in refusing to examine Stalinism in the intellectual framework established by Stalin's claim to be the successor of Marx, Engels and Lenin and the leader of the most advanced and enlightened political and social system yet known. Western scholars accepted Stalin's claim to be Lenin's successor, but characterized the system as evil rather than good. While never accepting Marxism and, in particular, the claim that the dialectical process stopped with the establishment of the socialist state, Western scholars with their theory of totalitarianism accepted the Stalinist belief that the system was permanent and to be changed from the inside only in its nonessentials.

The present generation of scholars, represented by the authors of this volume, reject this simplistic scheme and examine the question in a broader historical perspective. They wrestle with issues such as whether events are determined or fortuitous, whether the personality of the leader or the character of the masses is more important, and to what extent revolutions revert to the old order. These are the oldest questions but the best ones. It is the sophistication and thorough scholarship with which they are treated that makes this book so valuable.

Perhaps the flavor of the symposium can best be conveyed by a necessarily compressed account of some disputed points. In any examination of Stalinism, collectivization must occupy a central position, as it does in this volume. Robert Tucker argues that Stalin was driven by deep internal pressures to validate himself as a revolutionary equal to Lenin who had first raised him up but had then tried and failed to cast him down. Stalin was unique in defining the situation of 1925–1926 in eve-of-October terms. Stalin's statements accepting NEP as the continuing basis of state policy were mere lip-service because he was intent on making his own great revolution.

Moshe Lewin poses the theory that (to paraphrase an old aphorism) the faster you try to change, the less you do. In trying to build a structure more grandiose than any Russia had ever seen, with the old-fashioned artisans that were at hand, Stalin could not but make the new resemble the old in many of its features. But he did even more. By shifting his major basis of support from a vanguard party (or, in different terms, a group self-consciously countercultural) to a large bureaucracy that by its size and origins could only represent traditional habits and styles, Stalin risked becoming the creature of this bureaucracy. This he avoided by first making their tenure and then their very lives uncertain.

Stalin's decision to collectivize, according to Lewin, was ad hoc, a response to the crisis he had himself created and the need to retain the support of those sections of the party that hankered after, and could only be effective in, the rough and ready methods of the civil war period.

In Lewin's picture, Stalin miscalculated badly in his agricultural policy and then crashed from disaster to disaster in his desperate efforts to stay at the head of affairs. In Tucker's version, while Stalin did not plan all the steps of collectivization in advance, the need to carry out his October revolution is the most important source of his behavior.

These are two quite different approaches to the problem. Yet Lewin not only accepts the importance of Stalin's personality; he insists on it. Tucker, for his part, is fully aware of the great influence of the political and cultural milieu, and of particular events. But the difference is more than one of emphasis; it is a difference of approach. Tucker is a psychohistorican and Lewin is a social historian.

Another fruitful disagreement among the participants was the question of the connection between Leninism and Stalinism. As one would expect, the historians of the opposition within the Bolshevik party, McNeal for the Trotskyists, and Cohen for Bukharin, argue that Stalinism was an aberration. Their ground is history and ideology. Rigby, whose interest is the structure of society, argues that since the Soviet Union dispensed with the cult of personality after Stalin's death, mass terror was not "necessary" and therefore Stalinism was not. Kolakowski sees Stalinism as the legitimate offspring of Marxism; for Markovic it is a bastard.

The problem posed is familiar. Was Robespierre the legitimate successor of Rousseau? Was it Napoleon who carried out the true purposes of the French revolution? The generations of scholars who have grappled with these questions have not solved the problem but they have illuminated it. In its turn *Stalinism* illuminates its subject elegantly and wisely.

HERBERT S. DINERSTEIN

Johns Hopkins University

Political Brokers in Chile: Local Government in a Centralized Polity. By Arturo Valenzuela. (Durham, N.C.: Duke University Press, 1977. Pp. xiii + 272. $13.75.)

Political Brokers in Chile is primarily a descriptive study of Chilean local politics prior to the 1973 military coup. Part 1 focuses on party competition. At the national level, Chile's multiparty system was highly competitive. Did that competition characterize local politics as well? A comparison of the number of parties running candidates and the percentage of the vote garnered in congressional and municipal elections between 1935 and 1973 reveals that electoral cleavages were almost as pronounced at the local level as at the national level. That conclusion, derived from system-level data, is corroborated further by an examination of party competition at the municipal (commune) level across the universe of Chile's 276 communes in two elections, the 1965 congressional election and the 1967 municipal election. The question then is raised whether or not competition varied with the developmental level of the municipality. While somewhat more of the variance is explained in local elections by socioeconomic measures (14 percent) than in national elections (7 percent), the large portion of unexplained variance suggests that competition between parties was not limited to more developed communes. Examination of each party's individual vote strengthens that interpretation. Only Communist party support was significantly related to commune developmental differences. The absence of a strong relationship between socioeconomic measures and party vote leads to one further observation; namely, that political party support was not strongly tied to class differences.

The question suggested to this reviewer is, what then did explain party competition? That enigma is never fully resolved in a satisfactory and explicit manner. Instead, another paradox is probed. After a somewhat lengthy (and at times seemingly contradictory) discussion of changes in the jurisdiction of municipal governments over time, Valenzula tells us that even though municipalities still retained many important functions, the "stakes of local politics were quite meager" (p. 52) because resources were far too scarce to resolve problems adequately. Why, then, asks Valenzuela, did individuals compete so fiercely to obtain a seat on the local council? Perhaps, he hypothesizes, national political careers began in local politics. While investigation reveals that previous officeholding was helpful, though not necessary, for election to national office, in general, having served as a municipal councilor (*regidor*) was not important in determining whether or not a party would select an individual as a candidate for a national race. Thus, in all likelihood, people did not run for local office to launch a national political career.

In part 2, Valenzuela turns from the unanswered question of what rewards enticed individuals to seek local office, and focuses on the functions performed by the *regidores*, the dynamics of local political processes, and the vertical linkages binding local and national political systems. The description emerged from interviews with 15 deputies and 74 interviews with local officials in 14 municipalities. In brief summary, resource scarcity and the centralized nature of the polity explained the *regidores'* role. Because the government lacked funds to support its programs, individuals needed assistance in obtaining the services and benefits to which, by law, they were entitled. Local officials acted as brokers to extract resources from the center. They sought either to fulfill the personal (particularistic) needs of their constituents (e.g., dislodging a pension check from the appropriate bureaucracy—the *gauchá chica*) or, less frequently, to acquire monies for a community project. The local broker would seek out a national broker, usually a member of Congress of the same party, who, in turn, would respond either by introducing a private bill in the legislature or by pressuring the appropriate bureaucrat. In return, local politicians attempted to deliver votes for their party and politicians in national elections. Thus, while the politics of personal favors prevailed over national issues in local elections, the intersection of local and national arenas meant that political parties still were important at the local level.

At the conclusion of part 2, Valenzuela draws some generalizations and differentiates between what he categorizes as Chile's broker politics and both interest-group politics and patron-client politics. He asserts that goals in a patron-client system are particularistic in nature and processed through individualistic (face-to-face) interactions, whereas goals in an interest-group system are categoric and processed through collective bargaining between relatively equal power contenders in the policy process. In Chile, politics at the local level included both particularistic and categoric goals but both were processed through individualistic transactions—a brokerage system. At the national level, politics more closely corresponded to the interest-group model. Valenzuela points out that in the literature of political development clientelistic patterns often are associated with traditional societies and explained by reference to culturally determined values and attitudes. He argues quite cogently that "as one moves from the local arena to the center arena, one does not move from one culture to another, nor does one find more 'modern attitudes'; one simply finds different structures and issues of the political game" (p. 168). Structural characteristics (resource scarcity and political centralization) and not culture, he suggests, explain both styles of politics. In part 3 of the study he traces the historical antecedents of the Chilean polity, party system, and municipal government in support of that contention.

This study certainly stands as a definitive work on Chilean local politics. Its conclusions are based on competently drawn and voluminous data, both aggregate and individual; its sampling procedures create confidence in its representative nature. However, the theoretical content is relatively slight and somewhat inconsistently presented; after drawing a distinction between Chile's broker system and a client-patron system, Valenzuela then contrasts the national interest-group system with the local *client* system (pp. 167–68). The reader is left wondering whether any differentiation exists, after all, between broker and client politics or whether it is the explanation (structural versus cultural) that concerns the author. While the conclusions drawn in part 3 in support of a structural explanation may interest scholars of comparative politics, the detailed historical presentation will discourage those who have no substantive interest in Chile.

SANDRA POWELL

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German Political Systems: Theory and Practice in the Two Germanies. German Political Studies, Vol. 2. Edited by Klaus von Beyme. (Beverly Hills: Sage, 1976. Pp. 234. $17.50.)

Through the series of which this constitutes the second volume the German Political Science Association is seeking to "convince an increasing number of colleagues abroad that the work of West German political scientists is relevant to their own scientific concerns." It contains eight articles, mostly by younger political scientists whose work has not been widely published before in English, several of whom represent approaches which fruitfully combine Marxist and empirical analysis. The substantive focus tends toward West Germany and the European community, with the limited treatment of GDR topics not quite justifying the volume title.

Heide Gerstenberger offers a critical overview from a Marxist perspective of the work of some leading normative and empirical state theorists. She trenchantly analyzes the work of Nicholas Luhmann, Jürgen Habermas, Klaus Offe and Frieder Naschold. Her sharp pen pins the authors to their places in contemporary confrontation patterns; thus Luhmann is characterized as "strengthening the West German civil servants' self assurance, which in turn immunized them against expert disagreement and citizen protest" (p. 75). She is more indulgent in her treatment of the authors of the so-called "Stamokap" school.

The international relations articles include a stimulating contribution by Gerda Zillentin of Wuppertal University, who examines the validity of prevalent communist theories about the effects of EEC supranational integration on the viability of capitalism. She empirically refutes several of the theses, including those positing an increase in crisis-proneness and an increase in working-class solidarity. Rather, she anticipates that "social protest will not turn into the political form of a class struggle but will be processed or regulated administratively or politically." An article by Gerd Junne, also included, examines how the growth of international banking groups and consortia has affected state functions. He sees them as contributing to a reduction of the economic policy capabilities of most individual states.

Very informative also is Heribert Schatz' review of what happened in the decade since planning became respectable in the FRG. He demonstrates how quickly the peaks of planning enthusiasm flattened out again, and how the gains in rationalization produced by program planning clearly lagged behind what was anticipated by its initiators. Only in defense and education have planning functions remained organizationally distinct.

In his article on "Political Ideology, Dissatisfaction and Protest," Max Kaase reports on a study undertaken in the context of a larger international effort. He reports on his survey study of the relationship between political dissatisfaction and "potential for unconventional political behavior," develops a theoretical defense of this construct, and briefly describes a pre-test analysis. In another survey-based article, researchers from the Frankfurt Institute of Social Research report on how manual and white-collar union members evaluate the various goals their unions are pursuing.

East-West linkages and research are treated in articles by two other author teams. Ulrich Albrecht and Udo Rehfeldt from Berlin analyze the "Conditions for Economic and Technological Exchange in the Cooperation between East and West." Clemens Burrichter and Eckart Foertsch from Erlangen describe the embattled arena of GDR research in the Federal Republic. They list some 25 institutes that conduct such research, but will surely not win any prizes for their English style. In this and several other pieces the editors should have insisted on greater pains to achieve a higher level of readability.

There has been some question whether the rapid increase of West German political scientists has been accompanied by a commensurate increase in high quality research. This and other companion volumes should allow English-speaking readers to begin to develop their own opinions on this score. By and large, its contents would appear to be more varied, and not of a lower standard, than those of a volume of *Scandinavian Political Studies.* Like the latter, if also contains a bibliography of books and articles, in this case mainly of literature that appeared in West Germany in 1973–74, but the titles are not translated into English.

ARNOLD J. HEIDENHEIMER

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Mao Tse-tung in the Scales of History. Edited by Dick Wilson. (New York: Cambridge University Press, 1977. Pp. xii + 331. $19.95, cloth; $5.95, paper.)

This is no opportunistically conceived or hastily contrived anthology meeting the artificial demand for a "final assessment" of Mao Tse-tung, now that he has been at last obliged to leave China to her own devices. As the editor takes care to point out in the preface, for those who may not recognize the names, this volume is to be taken seriously not only because of the importance of its subject but also because of the quality of its contributors. They are—all of them—among the most distinguished scholars in the field, each of them, as Wilson says, having "devoted his or her entire life to modern Chinese studies."

But all these lives of study have not yet provided a consensus on the problem of Mao Tse-tung. Still, this is as it must be. Each of these ten essays addresses a different facet of Mao's thought and action over nearly 50 years of trial and error, glory and defeat. The inconsistencies are unavoidable and there are some rewarding common themes. One of these is the view that Mao felt himself to be a heroic figure, with strength and vision superior to ordinary men. Benjamin Schwartz sets the tone here, finding that Mao's poems assume "the eye of the hero leader" who like the legendary roc bird soars over and surveys the world of humanity. Schwartz links this with Mao's admitted early adoption of the self-image of a scholar, an act which he points out was, in pre-modern China, "fraught with deep significance. In undertaking it one took on the perspective of those with a responsibility for leading society" (p. 13). Even in his youthful passion for the popular novels, Schwartz maintains, Mao identified best with the heroism and cunning of the great generals and political leaders who were "definitely establishment figures."

Frederic Wakeman takes up the theme of Mao's establishment heroism and deftly develops it into an examination of two forms of Chinese patriotism emerging as the old dynasty collapsed: scholarly cultural loyalty on the one side and popular ethnic anti-foreignism, on the other. In his native Hunan, Mao was greatly affected by both but, according to Wakeman, came increasingly to identify national liberation with social revolution during the first United Front, and by 1927 made a personal decision to abandon scholarly populism in favor of the native peasant tradition of protest. This Wakeman says "was certainly a conscious transformation" in which Mao "embraced the lower classes to become more like them" (p. 242). From then on Mao's heroic mission was both class struggle and patriotic struggle combined.

But the theme of Maoist heroism achieves its full dimension only in the powerful essay by Edward Friedman, rightly saved for last. Friedman dwells not on Mao's personal aspiration to do great deeds, but on his exceptional risk-taking, even death-defying determination to see China resist all "supposedly statist imperatives" of the age, to achieve the end vision of genuine human liberation and a good and just society. "Mao preferred the possibility of liberation against conceding to any risk no matter how hypothetically large" (p. 317). And thus he could come to regard even the atom bomb as a paper tiger. "As the prophets of old, Mao declared that—at least inside China—justice was to be done even should the world tremble. Nothing was more important than fulfilling the human project" (p. 319). In an essay which incidentally launches a frontal attack on mainstream Western political science that "cannot be bothered" with the ethical problems of how to construct a more just society, and that has pusillanimously bowed before the pessimism of our era "which teaches not only that human problems have no solutions, but that those who insist on actively innovating and seeking answers are dangerous idealists" (p. 310), Friedman recalls not the potential disasters of Mao's way, but its profoundly constructive faith: "No obstructive mountain was so large that it could not be cleared away. Every evil had to be reconceived as an opportunity" (p. 304). Mao's heroism resides than not in any particular deed or project but in this "robust continuing quest to discover new practical modes to advance truly human goals" (p. 313), a quest denied and ridiculed by his century.

But what of the "mass line," what of democracy and popular participation in a system presided over by such a hero? The theme of leader and led, party and masses, receives much thoughtful attention from several contributors. Friedman points out that Mao did not invent solutions out of thin air, that he depended on the creativity of local citizens. But Stuart Schram argues in an essay which will not disappoint those who over the years have followed his ideas about the thought of Mao Tse-tung, that in Mao's view the elaboration of correct decisions still must take place at the Center, and that "for better or worse, Mao's approach to politics remained to the end of his life far more thoroughly impregnated with Leninism than many people (myself included) were prepared to admit a decade ago. . ." (p. 47). Pischel, in an elegant and subtle essay which deserves much more attention than can be given here, addresses this issue, saying, "All his life Mao wanted to be a 'teacher'—not a 'master'; just a teacher" (p. 172). Since the best teachers are also continually learning from their students, Mao, she suggests, viewed himself and the party as "primarily teachers but also pupils" of the masses.

There is much more here. John Gittings has produced for this volume that seemingly rarest of all commodities, a thoroughly intelligent and engrossing essay on Chinese foreign relations. He faults Mao for his tendency to view the whole international scene as a function of superpower activity and, concentrating on the contradiction between "socialism in one country" and the cause of proletarian internationalism, reminds us of several of the uglier examples of recent Chinese pursuit of state-to-state relations "at the expense of other people's revolutions" (p. 264).

There is an essay by Jacques Guillermaz on Mao's military theory and practice, and one by Michel Oksenberg on Mao's political style which is brimful of important insights. Chief among these, probably, is Oksenberg's observation that Mao tended to be less concerned with immediate power balances among allies and enemies than with developing trends in the balance. That is, he was prone to conceive of political forces not as "strong" or "weak," but as "weak-and-becoming-gradually-strong," or as "strong-and-possibly-becoming-rapidly-weak." Oksenberg also provides a fine explication of Mao's method of scanning a political situation to find the "major contradiction" and then moving deliberately to exacerbate it.

There are shortcomings as well. Christopher Howe and Kenneth Walker have contributed a valuable reference work on Mao and Chinese economic development policy, offering one of the best and clearest accounts available of the rise and fall of the Great Leap. But the essay is marred by numerous questionable interpretations and a not very productive preoccupation with assigning credit and blame. (Thus Mao is praised for his responsibility in pressing China's population control program, and blamed for erratic performance in the steel industry.) It would help here to distinguish between, rather than to conflate, the concepts of "self-reliance" and "self-sufficiency"; and, as is also true of Oksenberg's essay, using the simple distinction between individual and collective material incentives would have prevented what seems to be a misrepresentation of Mao's views on the important role of material incentives in socialist development.

In a sensitive essay near the end, Wang Gungwu suggests that it was Mao's most "un-Chinese" characteristic, his haste, his unwillingness to settle for steady progress, that finally caused his "ambitious revolutionary vision" to run afoul of "China itself: the 900 millions, the deep political and cultural heritage, the people's pervasive . . . urge for order after a century of [disorder] which grew stronger with each decade" (p. 297). This view seems to be shared by some other contributors and it leads Wang to the mournful conclusion that Mao's project of revolutionary social transformation for China cannot but now be abandoned. "If it could not be done while Mao was alive, what hope is there that it can be done after he is gone?" (p. 298) It is left to Edward Friedman to offer a tentatively more hopeful thought. At this point in history, he maintains, the final verdict on Mao cannot yet be written. Whether Mao's life work of setting Chinese society on a continually self-revolutionizing course was or was not successful can only be told in the future, when the hero's presence has faded. "The real test for his whole innovative approach comes only with his death when he can no longer intervene" (p. 315).

VIVIENNE SHUE

Yale University

Housing and Housing Policy in the U.S. and the U.K. By Harold L. Wolman. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. xv + 125. $13.00.)

Harold Wolman's book is useful, well written, and comprehensive—yet admirably succinct. Comparing housing policy in the U.S. and the U.K., it is unpretentious in style and aim and succeeds in what it sets out to do. Intended more for the practitioner or policy analyst than for the theoretically minded social scientist, the book nonetheless contains analysis crucial for theory.

The work contains five sections particularly useful to academic analysts of housing policy. They are Wolman's analyses of: (1) the different assumptions underpinning national policies and of the ways in which such assumptions are expressed in actual programs; (2) the general notions contained in—and the ambiguities surrounding—the British conception of "housing as a social service"; (3) the costs and benefits of rent control, including its impact on the private rental sector; (4) the problems attendant on "municipalization"—local government control of the rental housing stock, including that which is now private; (5) the responsibilities for housing which local governments exercise in their respective countries.

A careful comparison of a specific policy area such as housing serves as a useful illustration of general differences among countries in political culture and social structure. Wolman specifies the links between such broad categories and specific policy approaches in a way that is more careful, systematic, and thoughtful than usual.

Wolman's book does not offer any new ways of thinking about "public policy" as a generic category and, I suppose, could be faulted on that count by those who believe that all policy studies should contribute directly to theory. The book does offer well-organized information and analysis which can assist those interested in housing policy to think about alternatives and options tried elsewhere. Few comparative studies provide such a foundation.

Further, Wolman's analysis is based on a sound understanding of the technicalities of the housing market—technicalities which he explains so lucidly that they are readily comprehensible to the lay person. He does not draw his conclusions haphazardly, and his analysis convinces one that "technicalities really do matter" in determining (and understanding) the final outcome of any housing policy.

One of the main merits of the book is the clarity and insight with which the author discusses the divergent policy implications of the "housing as an investment" and the "housing as a social service" concepts—particularly as they pertain to the problem-ridden private rental sector. U.S. policy, to a large extent, embodies the former concept, policy in the U.K. the latter. "Americans are expected, if at all possible, to provide for their own housing" (p. 1)—and the government does not take general responsibility for the quality and costs of such provision. In contrast, "the United Kingdom has accepted that shelter arrangements—including those provided through the private rental sector—should be a primary concern of society. The fact of private ownership does not negate the state's responsibility in this area, but rather may call forth more explicit social service approaches" (p. 109).

The policy consequences flowing from such different conceptions of the proper role of housing in the economy show up clearly in taxation policy and policies related to tenant rights. While in the U.S. private rentals are treated well by tax legislation, in the U.K. they are not. Tax disincentives are there coupled with rent control laws and security of tenure provisions which give tenants virtually absolute security in their apartments. British landlords may consequently find it preferable to convert their properties for sale to prospective owner-occupiers. This process has led to a sharp reduction in the size of the private rental sector and to the practice of keeping portions of buildings empty in order to avoid the problem of "sitting tenants" at the time of sale. Naturally, empty buildings command a greater market value than those occupied by "tenants with tenure." This process has, of course, increased the stress on a housing market already characterized by chronic shortage.

Wolman's conclusions are clear, if uncomfortable. If a private rental sector is to survive, governments must recognize the need for an adequate return on investment, irrespective of their commitment to housing as a social service. Investment returns must be carefully weighed against tenants' rights. Such accommodation need not imply freedom to charge exorbitant rents: it is, in fact, vital to ensuring access for lower-income groups to the private sector. Such access is increasingly restricted by the diseconomies imposed by statute. As Wolman says:

> Something must be done to lower the rental cost *and* improve the quality of lower-income private rental housing. The quality of this housing can be improved—or prevented from deteriorating in the face of lower rent revenues—only in a context that provides the owner of such housing a reasonable return on investment. This clearly will require some form of state subsidy (to meet the owners' needs) and quite probably some form of state control (to assure the tenants' needs are being met). The British have concentrated on the latter to the exclusion of the former.

Those who shape and study British housing policy seem increasingly to share Wolman's view. In 1977 the Labour government started, albeit tentatively, to reexamine its approach to rental housing. As *The Economist* noted sardonically, the government "began gingerly testing public reaction to a sane policy for lifting landlords out of the caste of untouchables and saving what is left of private rented housing" (*Economist*, May 2, 1977, p. 18).

Those engaged in reevaluation of housing policies will surely be helped by reading Wolman's work and their thinking will be clarified by the comparison he draws. American policy makers will benefit no less than their British counterparts. Academics also may learn something from the book.

ALBERTA SBRAGIA

University of Pittsburgh

China's Higher Leadership in the Socialist Transition. By Paul Wong. (New York: Free Press, 1976. Pp. 310. $16.95.)

In this ambitious work, Paul Wong develops a critique of previous studies of elites in the People's Republic of China and offers a refreshingly different approach which incorporates the need to place elite studies in the context of evolving political events and which also attempts to evaluate the real "power" possessed by members of various elite groups. The results of his analysis are then discussed in relation to the set of problems, common to all Marxist-Leninist societies, associated with the "transition to socialism." In effect, he uses the composition and circulation of elites as a means of evaluating the relative success of China's socialist transition. China, it is argued, is more or less socialist according to whether power is more or less concentrated in the hands of the same individuals over time. Less concentration points to greater success in achieving socialism.

In developing his analysis, Wong notes the existence of tensions between formal organizational networks consisting of the party, government, army, and mass organizations and functional "systems" such as foreign affairs which involve more than one component of the organizational network. Different categories of elites are then constructed on the basis of the number of such overlapping organizations in which individuals have membership, how long they have occupied their positions, and factors related to age, education, and foreign experience. We are thus presented with both a rank-ordering of elites and a respectably detailed statement of their composition. Finally, the analysis is set within the time frames 1949–1958, 1959–1964, 1964–1966, and the period from the Great Proletarian Cultural Revolution (1966) through 1975. This strategy is adopted to provide some notion of the extent to which elites have changed in response to different orientations regarding both the content and implementation of public policy.

On the basis of meticulous quantitative analysis, Wong reaches several important conclusions. First: progression upwards through the levels of the elite structure from 1949 until 1966 was essentially bureaucratic; advancement (and demotion) occurred in response to demonstrated ability and political necessity. Second, that while the size of the higher leadership increased as a result of the general expansion of the political and economic superstructure, most members of the higher political leadership continued to hold positions in only one of the overlapping organizations between 1959 and

1966. Despite this, however, a small number of individuals held membership in several overlapping organizations; from this it is to be inferred that China's leadership was really quite concentrated. Finally, Wong asserts that by the 1960s power structures had rigidified, thus limiting the ability of outsiders to penetrate the system and incidentally slowing the transition to socialism. It is good to have empirical verification of what have heretofore been essentially intuitive evaluations of leadership trends.

However, it is in the discussion of the impact of the Cultural Revolution and its implication for the transition to socialism that the quality of Wong's analysis deteriorates. He asserts that owing to purges since that time, the leadership has become much more broadly based with representatives of different strata playing increasingly important roles. The Red Guards, Three-Way Alliances, and the Revolutionary Committees were all vehicles by which the masses overthrew the state and thereby helped to enhance the socialist nature of China's socioeconomic-political system. This argument poses certain problems.

Wong assumes the existence of qualitative differences in notions of acceptable mass political behavior among competing sectors of the higher leadership. While it is true that different sides in the Cultural Revolution advocated different *styles* of elite-mass relations, there is no evidence to support the contention that the leading roles of relevant elites were to be challenged. For example, in the post-Cultural Revolution era the party continued to occupy primacy of place. The Cultural Revolution represented a new mode of elite competition. Accordingly, while new individuals came to occupy important positions, the pattern of control by a relatively small group with membership in several organizations endured. While more workers and peasants achieved membership in the higher elite—and the number mentioned by the author is hardly significant—real power continued to be exercised by those who had emerged as victors in the struggle. In effect, one state was replaced by yet another state and, judged by Wong's own criterion, the new state was not more socialist than the one it displaced.

Apart from over-interpreting the significance of changes in the composition of China's higher leadership after the Cultural Revolution, the author also fails to deal with the quality of motivation of those individuals who supported Mao and the radicals. To say that "the Mao faction essentially coalesced with youth, workers, and peasants in order to regain control over the organizational system. . ." (p. 264) is to oversimplify an extremely complex problem. Even if this assertion is taken as valid, it is still necessary to ask why different kinds of people joined the coalition and to determine the socialist potential of their collective motivation before one can generalize about the impact of the event upon the transition to socialism.

In sum, while Wong has done an admirable job of analyzing the composition and circulation of China's higher political leadership, he has not demonstrated the utility of these variables as indicators of movement in the transition to socialism.

RONALD N. MONTAPERTO

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Communism in Indochina: New Perspectives. Edited by Joseph J. Zasloff and MacAlister Brown. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. xi + 295. $16.00.)

The dozen papers in this volume are written by some of the leading American experts in the field. Although they were written before the Communist assumption of power in Indochina (three received some revisions), some are extremely useful in understanding these Communist movements and the war and all have some value. An introduction also serves as an epilogue on the Communist take-overs and raises general questions about future developments. The papers were prepared for a September 1974 conference of an area panel of the Southeast Asia Development Advisory Group (SEADAG) of the Agency for International Development (AID) and the Asia Society.

Over two-thirds of the book treats Vietnam and four of these contributions are of unusual significance. William Duiker offers a concise but balanced study of the rise of Communism up to 1954 and Carlyle Thayer covers southern revolutionary organization and the role of the Lao Dong party from 1954 to about 1969.

David Elliott analyzes the DRV agricultural cooperativization program as an "instrument of integration," effectively generating and channeling political participation. The party could not do this more directly (as consonant with a common interpretation of Samuel Huntington's model) mainly because it was small, comprising less than one percent of the village population. With this formula, the party did not have to impose elitist goals of development. The cooperatives offered material incentives and livelihood gains linked with expanded local-level popular participation so that the party's "revolutionary" ideas and aspirations were supported—and even pushed faster from below than the provincial cadres sometimes wished—

"by traditional ideals as much as by 'modern' ideology," Elliott claims.

William Turley's important though sometimes less than incisive paper deals with a range of DRV military theories and strategies in the context of the PAVN's (the army's) need to maintain revolutionary class consciousness, particularly after the modernization and regularization programs from 1958 onward, and to deny politico-military victory to its American foe. Turley emphasizes that "compromise and incrementalism have been the hallmarks of the PAVN's political role" as Elliott holds regarding cooperativization.

Three articles examining DRV diplomatic and military strategies have become dated in some respects, but offer useful thought drastically contrasting perspectives. Gareth Porter claims the Communists from January 1973 on had a two-track strategy potential: they could either conform to the cease-fire, relying on political struggle and avoiding the risk of renewed U.S. bombing of the DRV; or, if the agreements were not implemented, they could shift toward a firmer military stance. Allan Goodman, however, contends that the DRV, influenced heavily by its previous extensive and dolorous negotiations experience, undertook them "only to facilitate military victory." Paul Kattenburg sees DRV policy as somewhat more flexible; he maintains that "DRV leaders may well have concluded that a final, crushing assault can be conducted later with relative international impunity," but he can envision other DRV approaches too if there were serious implementation of the cease-fire, for example.

The section on the Khmer resistance by Sheldon Simon, Donald Kirk, Milton Osborne and Peter Poole is interesting, if sometimes dated. Some themes developed jointly by some of these writers are as follows: The DRV was not eager to see its Khmer Rouge allies triumph in Cambodia before the Communists won in Vietnam. Friction between the North Vietnamese and the KR lessened after Vietnamese participation in the fighting declined from mid-1972 on (and the withdrawal of most Vietnamese cadres after early 1973, according to Simon). Kirk reminds us that the surprising political violence reportedly used by the victorious Communists goes back to 1972, increasing greatly in about 1974—due, he believes, to the KR's mounting frustration over difficulties in mobilizing the people for a new national revolutionary purpose.

The article on Laos by Joseph Zasloff and MacAlister Brown focuses on the ill-fated Vientiane government-Pathet Lao coalition preceding the 1975 Communist ascendance to outright control but contains useful analyses of PL behavior and ongoing problems such as refugee disposition and economic viability.

One of the questions raised by the papers as formulated in the introduction has been answered at least partially by now, i.e., "whether radical social restructuring and equalization or national independence and development should require top priority." (Actually, this dichotomy is no longer very apt. All three countries place high priority on independence, all strive for economic development—with Cambodia most radically committed to self-reliance—and even southern Vietnam has seen a stronger emphasis on social restructuring than was indicated earlier.) However, much work remains to be done toward answering other questions posed by this useful book concerning relationships "between Communist parties and states in Indochina," issues of "central control versus local autonomy within the Communist movements," and decision-making processes within the parties, including the influence on these of ethnic, religious and other interest groups.

JOHN C. DONNELL

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Political Strategies for Industrial Order: State, Market, and Industry in France. By John Zysman. (Berkeley: University of California Press, 1977. Pp. ix + 230. $12.75.)

This book offers a fascinating account of the unsuccessful French effort to develop an independent national electronics industry in the 1960s. The management of technological change, it argues, is a problem which all advanced industrial states confront. But the solutions these states offer will differ because the management of change is politically determined. This emphasis on the political causes and consequences of technological change is useful. It points to the inescapable political distribution of costs and benefits in society which accompany technological progress.

More importantly, this book is also a brilliantly conceived analysis of the internal organization of the firm, of the relations between business and government and of French conduct in world markets. Chapter 2 distinguishes between the technological imperatives of the science-based firms in the electronics industry and the culturally rooted organizational patterns that typify French institutions. Building on the work of Woodward and Crozier, Zysman focuses on technology (which requires decentralization, easy horizontal interactions between tasks and fluid personal relationships) and culture (which in France demands centralization, vertical rule-based interactions and impersonal relationships) as the two main determinants of organizational change. A survey of the structure of 18 electronics firms and case studies of organizational change in 4 of these firms, presented in chapter 4, finds the pattern of change to be more complex than is predicted by either the technological or the cultural explanation. The general movement away from centralized organizations was a response to the pressure of the technological task and was not blocked by an aversion to face-to-face conflict. But "change did not occur in all firms, nor could it have been said to be fully satisfactory in very many. In the process toward decentralization, the heritage described by Crozier left its traces" (p. 104).

Chapters 3 and 5 analyze changes in business-government relations. In the Fifth Republic the state has lowered the barriers which traditionally had protected French industry. The state bureaucracy has shifted away from weak arbitration between competing industrial interests to energetic entrepreneurship. Defining "success" as the establishment and maintenance of an industrial structure satisfactory to the state, Zysman documents briefly the success of government policies in the oil and steel industries where massive, centrally controlled investments were the appropriate response. Why did a similar policy fail in the case of electronics? Although considerations of costs were not unimportant, Zysman singles out the indirect effects of state intervention, for that intervention perpetuated the traditional protectionist posture of French firms by encouraging parts of the electronics industry to look to the state for subsidies and protection against the competitive pressures of the market. Furthermore, for different groups of firms, and even divisions within firms, Zysman shows that the organizational requirements of negotiation with state bureaucracies induce a degree of centralization incompatible with the norms of efficiency in the electronics industry. "Whatever its primary purpose, French support [of the electronics industry] . . . subverts its own goals" (p. 152). Part 3 of the book synthesizes and broadens further the argument of the earlier chapters by examining the importance of political culture and international markets on the behavior of firms and governments.

This tightly argued book is exemplary in how it uses a small but carefully selected data base to illuminate large theoretical questions of political importance. However, it is decidedly not exemplary in its technical production. Four typographical errors in eight lines (p. 150) are indicative of the publisher's prefunctory copy-editing of the entire manuscript. The two central hypotheses of the book invite two general comments. (1) For two reasons it remains unclear whether the book follows the right research strategy in its test of the relative impact of technology and culture on organizational structure. First, a cross-national study of change in corporate structures (referred to by the author and summarized by Bruce Scott in the *Harvard Business Review*, March-April 1973) concludes that in the 1960s British, West German, Italian and French corporations have all moved rapidly from a hierarchical to a divisionalized organizational structure. Wouldn't it be preferable, then, to construct one overarching "European" explanation of organizational change rather than a particular explanation for each of the four countries? Secondly, to the extent that the book analyzes the way in which change came about rather than the mere fact of change (in Crozier's terminology "the pattern of change"), the retrospective nature of the evidence and the limited number of firms make for an uncomfortable arbitrariness in interpretation. On p. 165, for example, organizational change is described both as an "unexpected thunderbolt" (when it is imposed from the top level of the organization) and as a "war of attrition" (when it is resisted by lower layers of the organization). Graphic prose is no adequate solution to the familiar problem of distinguishing between a bottle which is half empty and another one which is half full. (2) The book is self-conscious in its exclusion of any consideration of the capitalist character of the French state. Remarkably, for a book on economic policy, the index has not a single entry for "unions," "workers," "labor" or "class." This, of course, is no inadvertent oversight. It is partly due to the highly skilled labor force which distinguishes the electronics industry and producer good industries more generally. But this lacuna is also a consequence of the theoretical focus on *organizational* structure and change. Like works of industrial relations literature, this book is based on the premise that institutional forms contain and shape the capitalist system. And unlike the Marxists, the author defines "structure" in institutional rather than class terms (pp. 10–12). The premise of the entire analysis is, in fact, that the business firm and the state bureaucracy are two distinct organizational actors. Yet one of the central findings of this book (corroborated by Friedberg's *L'état et l'industrie en France*, Berlin: International Institute of Management, 1976) points to an interpenetration of electronics firms and state bureaucracy which is so extensive that it becomes highly implausible to view them as two distinct actors (pp. 59, 65, 89). The implications for the book's central argument are limply acknowledged only once (p. 175) since the theoretical framework simply cannot accommodate them. This is no small matter for a political analysis of a country whose Left stands at the threshold of power.

This book is a brilliant addition to a distinguished number of studies of French economic policy. The strength and endurance of the French state, it seems, provides a compelling focus for an analysis of the political determinants of economic policy. Zysman's analysis is so powerful because it succeeds in always tailoring its discussion to this broader, underlying theme.

PETER J. KATZENSTEIN

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International Politics

Remaking Foreign Policy: The Organizational Connection. By Graham Allison and Peter Szanton. (New York: Basic Books, 1976. Pp. xiv + 238. $10.95.)

As indicated by its title, this volume argues that American foreign policy must be remade. Graham Allison and Peter Szanton start from the premise that "U.S. performance in foreign relations has fallen well below the standard the nation requires." Their major thesis is that "poor organization is an important source of that failure" (p. 25). The structure of foreign policy decision making, which came into being over three decades ago, is viewed as obsolete. Foreign policy today, they argue, suffers from numerous defects, chief among which are the fragmentation of policy making, the sacrifice of broad considerations for narrow military or economic goals and the failure to meet the requirements of global interdependence.

To overcome these flaws Allison and Szanton propose a variety of reforms focusing on the presidency, Congress, the Department of State and the intelligence community. They propose, *inter alia:* abolition of the National Security Council (NSC) and its replacement by an executive committee of the Cabinet (ExCab); a strengthening of the president's staff with four principal assistants; presidential submission of a biennial statement of the United States' foreign commitments to Congress to be debated by Senate and House Committees on Interdependence; five-year congressional authorization for defense and foreign aid; making the advocacy of foreign policy positions the central role of the Department of State; removal of the three service chiefs from the chain of command; and breaking up the Central Intelligence Agency (CIA) into two new organizations: a Foreign Assessment Agency to handle only analysis and a Special Services organ to engage in clandestine activities and covert operations; an assistant to the president for intelligence would replace the Director of Central Intelligence (DCI).

Their proposals are thoughtful, informed and provocative. Though this is not a scholarly book, it reflects a thorough familiarity with the scholarly literature as well as extensive involvement in government reform. Many of the authors' specific ideas are drawn from their work in the "Murphy Commission" (Szanton as Research Director and Allison as principal researcher for defense and arms control studies). Most of these proposals deserve serious consideration. Perhaps the most controversial, and to me the least useful, are the proposals advanced concerning the CIA. The benefits of combining intelligence analysis with clandestine collections seem to outweigh whatever advantages a separation of function would bring. Sound analysis clearly requires the information gathered by secret operations, and covert activities should be undertaken only when supported by the judgments of sound analysis. During the first 14 years of the CIA's existence these two functions were housed in separate buildings and that alone tended to impede communication between personnel responsible for each activity.

Though there is merit in most of Allison and Szanton's proposals, their arguments are not totally persuasive. Their underlying premise—that American foreign policy has been fundamentally inadequate—is questionable. The examples of recent failures in foreign policy, though telling, hardly prove the case. Indeed, one of their examples of inadequacy, American failure to create a new regime for the Panama Canal, is now in the process of being rectified. One can, however, find sufficient failures of performance in recent United States policy to concede the point.

But Allison's and Szanton's argument for organizational reform is flawed on two more basic counts. First, their analysis exaggerates the impact of bureaucratic structure upon foreign policy decisions. It also underestimates the extent to which foreign policy is molded by the international system. Organization matters! Yes, but how much? It is difficult to believe that all the proposed reforms, even if implemented, could or would have kept the United States out of Vietnam or prevented American vulnerability to the OPEC countries. Secondly, Allison and Szanton underestimate the primacy of politics—not just bureaucratic, but also partisan, interest-group, ideological and electoral—over organization. The organizational structure itself reflects the complex influence of individuals and interests that are unique to every administration. Structures are not easily reformed along rational lines because inevitably vested interests and powerful personalities adversely affected by change will maneuver to subvert the effort.

Thus, Allison and Szanton entertained unrealistic expectations for bureaucratic change in the new administration. Writing shortly before the 1976 election, they assert that "the period just ahead" offers the best opportunity in 30 years for important organization reform. There was an opportunity to bring the executive and legislative branches under the control of one political party which they consider useful for integration and coherence of policy. Democratic control of the legislative and executive branches of government has brought some change but nothing as drastic as urged in this volume. As advocated by Allison and Szanton, Congress has restored the president's authority to reorganize the executive departments (subject to congressional veto). So far President Carter has used his authority under the Reorganization Act of 1977 to cut back the White House staff for economic reasons. Though Carter has endorsed a central objective of this volume, namely a strengthening of cabinet government, he also reaffirmed the previous role of the NSC. Similarly, recent reorganization of the intelligence community conforms to only a minor part of the author's prescriptions. Carter's Executive Order of January 24, 1978, establishes restrictions on intelligence activities threatening civil liberties, but it also increases the authority of the DCI over all intelligence functions.

The problem of reorganization of the foreign policy apparatus, however, is by no means settled. Those in government and academia concerned with the issue would do well to consult this volume.

JOSEPH L. NOGEE

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Developing the ICBM: A Study in Bureaucratic Politics. By Edmund Beard. (New York: Columbia University Press, 1976. Pp. xi + 273. $15.00.)

Edmund Beard seeks to explain why responsible U.S. officials permitted the "missile gap" to develop when they were so obviously committed to maintaining military superiority over the Soviets. In answering the question Beard applies a bureaucratic politics framework to information gathered through a review of official records, interviews, and an analysis of secondary sources.

The book focuses on the period between 1946 and 1954. In 1954 decisions were made to accelerate the almost dormant U.S. ballistic missile programs. Had those decisions been made a few years earlier Beard is certain that the national trauma precipitated by the Soviet Union's launching of Sputnik and the military overreaction which it generated could have been avoided.

The culprit of the story is an air staff dominated by the advocates of manned bombers. Under the air staff's directions, the air force successfully fought for jurisdiction over strategic missiles in the period before 1954, but failed to exploit effectively the near-monopoly obtained in this field. While senior air force officers recognized the potential strategic value of ballistic missiles, most preferred the air force to first develop its bomber force and then explore cruise missile technology before moving on to ballastic missiles which were viewed as a technically difficult, long-range option. The Air Research and Development Command, the home of most of the ballistic missile proponents within the air force, was prevented by the air staff and rival support commands from receiving sufficient funds to prove the early feasibility of ballistic missiles.

The air force ballistic missile programs remained stalled, according to Beard, until the new civilian leadership of the air force seized the decision-making initiative from the air staff at the start of the Eisenhower administration. Buttressed by the advice of RAND and civilian scientists, the air force secretariat forced an acceleration of the ballistic missile programs. An autonomous subdivision, the Western Development Division, was created within Air Research and Development Command and given authority to control all air force planning and research related to ballistic missiles. The air staff reluctantly acquiesced—apparently fearing that any attempt to thwart the secretariat's design for the acceleration of the ballistic missile programs would lead to the removal of the program from air force jurisdiction and their transfer to a Manhattan-like project office.

The book suffers mildly from some errors of omission. Although Beard correctly dispels the myth that the feasibility of ballistic missiles required a breakthrough in warhead design, he fails to examine critically the view that second-generation missiles were dependent upon a breakthrough in propellants. Similarly, Beard explores at length the conflict between the air force and the army for jurisdiction over land-based missiles, but ignores the collusive relations that existed between the air force and the navy in that conflict.

More serious problems, however, develop when Beard attempts to draw conclusions from his well-told case history. Beard claims that his work on ballistic missiles, like Morison's history of continuous aim gunnery and Katzenback's study of the horse cavalry, demonstrates that the military is resistant to technological change. It hardly seems accurate, though, to accuse the post-World War II military of Luddite tendencies. Anything else, perhaps, but not the failure to be innovative. The exploitation of ballistic missile technology was retarded more by the monopoly the air force managed to obtain over its early development rather than inaccurate evaluations of its potential strategic value. Beard argues that what is needed is an independent agency within the defense establishment to aid in the assessment of opportunities for weapon innovations. But three such agencies already exist: the army, navy and air force. Once interservice competition was unfettered in ballistic missiles all the alternatives and their limitations were rapidly revealed. What is lacking and what is apparently extremely difficult to obtain is the knowledge of when to use bureaucratic competition and when to restrain it in the development of weapons.

HARVEY M. SAPOLSKY

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The Origins of International Economic Disorder: A Study of United States International Monetary Policy from World War II to the Present. By Fred L. Block. (Berkeley: University of California Press, 1977. Pp. xii + 282. $14.00.)

Most studies of international economic relations are written by economists, retired practitioners, and formal officials. Too often these works become entangled in the complexity of jargon and techniques of international economics, confuse all but a few specialists, and fail to ask, much less answer, central political and social questions concerning the development and operation of economic interactions among nations. Sociologist Fred L. Block has explicitly and quite successfully foresworn mechanistic detail, providing instead a thoughtful, readable, radical-revisionist analysis of American international economic policies and policy making since 1900. He concentrates, particularly, on the origins and early development of the Bretton Woods system in the industrialized nations.

Block is interested in the "ways in which specific international monetary arrangements both reflect and influence the distribution of political-economic power among major capitalist countries" (p. 1). He argues that the "capitalist class" in the most powerful capitalist nations generally favors a more open international economic system which allows them to exercise leverage over other nations, freely pursue profit opportunities beyond their borders, and resist working-class demands for higher wages and improved social services. Therefore, at the close of World War II American capitalists hoped to establish an open economic order and prevent the rise of Keynesian "national capitalism" which threatened to increase state intervention and planning to maintain full employment and high industrial productivity. Block believes that even after the intensification of the cold war, American foreign policy makers were more concerned about the specter of rising "national capitalism in Western Europe than they were with a possible invasion by the Red Army or successful socialist revolution" (p. 10).

After reviewing the demise of the gold standard, Block carefully examines the roots of U.S. international economic policy between 1940 and 1958 and shows how the free market orientation favored by the Department of State gradually came to dominate the views of the Keynes-influenced U.S. Treasury Department. He compellingly delineates the strong economic biases held within the U.S. government and demonstrates that these were often critical in formulating international economic and political policies. His sections on economic bases of U.S. policy at the formation of the Bretton Woods system, during the negotiations surrounding the British loan, at the emergence of the Marshall Plan, and during the long push toward European currency convertibility unearth no new information, but present the revisionist perspective with clarity and sophistication largely absent from previous radical interpretations of this policy-making period.

The final third of the book, which describes the decline of the dollar and the collapse of the Bretton Woods system in the 1960s and 1970s, is straightforward, but too short and less interesting. Perhaps an over-ambitious editor slashed more than was desirable. It becomes extremely difficult to illustrate how the capitalist classes influenced the direction of American foreign economic policy when almost no mention is made of the activities and policy preferences of multinational banks and corporations, particularly since private sector behavior and perspectives shifted substantially after 1969.

Although Block's work makes a positive contribution to our understanding of the origins of the postwar economic system, several problems remain. "Monetary policy" seems sometimes to be subsumed within "economic policy" and at other times the terms are used as synonyms. Evidence is scanty regarding the mechanisms which corporate capitalists employed to insure that government officials would pursue policies they favored. Another difficulty is that Block's critique is focused entirely against a mercantilist perception of economic history. The contributions made by political scientists such as Robert Russell, Robert Keohane, and Joseph Nye in analyzing international monetary politics from transnational and transgovernmental perspectives have not been integrated into this study. As a result the link between theory and analysis is more tenuous than might have been possible.

Block concludes that major monetary reform is necessary but highly unlikely and predicts "that the international monetary system will suffer severe and chronic crises in the years ahead" (p. 221). He suggests that the widespread emergence of democratic socialism could ease or perhaps even resolve many of the contradictions in the existing international economic system. Dissenting practitioners could point out that the monetary system has been remarkably calm and orderly since July 1973, considering the major shocks it has experienced and could argue that the recent partial depoliticization of monetary relations coupled with the revitalization of the International Monetary Fund may allow the international monetary system to right itself and function effectively for many years to come. Which future is more likely is unclear. It is clear, however, that Fred L. Block's analysis deserves to be read and considered seriously.

JONATHAN DAVID ARONSON

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Simulated Worlds: A Computer Model of National Decision-Making. By Stuart A. Bremer. (Princeton, N.J.: Princeton University Press, 1977. Pp. xvi + 249. $15.50.)

The purpose of *Simulated Worlds* is to "construct and evaluate a macro-level theory of international politics," to state the theory as "an operating computer simulation model," and to "discover the implications of alternative theoretical formulations" (p. 3). Following his introduction, Bremer discusses the model, assigns values to variables and parameters, reports suggestive sensitivity analyses, and examines the validity of the model.

One of the benefits of a computer model is that it forces clear specification of theoretical statements. Much of this clarity is lost in the presentation of the model in chapter 2. Verbal descriptions, flow diagrams and equations form the basis of this chapter, but I frequently found it difficult to reconcile the information provided in these different ways (e.g., pp. 54–55). There is no complete flow-chart of the model, no computer program, and no single listing of the equations and variables. Furthermore, some terms in equations are not defined adequately (e.g., the substantive interpretation for a_{17} on p. 38); the equations and steps in the flow-charts are not numbered for clarity; and the mathematical notation is confusing. Unfortunately, after carefully reading *Simulated Worlds,* I could not fully understand the computer model, and thus could not understand the "theory."

A primary reason for building models is to make it easier to determine the ramifications of a set of assumptions. Therefore, we may ask what is learned by deductions from the model in *Simulated Worlds.*

Part of the model focuses on the decision-making environment and concerns itself with a tight bipolar international system with five nations, a national economic system and a national political system. It is "borrowed essentially from INS [Inter-Nation Simulation]" (p. 29). The other (larger) part of the model "specifies how a national decision-making unit attempts to achieve its goal in a changing environment" (p. 29). In chapter 2, which presents the basic model, there is no citation to any literature published in the last seven years.

The real basis for the model is found in literature published at least ten years ago. The concerns dominating the recent literature are reflected in the model only a general way. Regrettably, the deductions from the model do not reveal the logical consequences of statements contained in recent literature.

All models are, to a greater or lesser extent, representations of the world. The model in *Simulated Worlds* is representative to a lesser extent; the nations and systems are hypothetical and are given names such as Nation 1 and System 1. The deductions from the model take the form of values that are generated for variables but there is no attempt at direct measurement of initial values for model variables or systematic estimation of model parameters from real-world data. The initial variable and parameter values are assigned arbitrarily by the author and the model does not lend itself to meaningful sensitivity studies involving counterfactual analysis or simulation of the consequences of alternative scenarios for the future for specific real-world entities. The model differs significantly from a computer model built on real-world data. Deductions from the model do not help us understand very much about specific real-world entities.

There must, of course, be room in the research process for the use of any assumptions of interest to the individual scholar (including arbitrary variable and parameter values). The real test is whether the deductions from the assumptions tell us something interesting. Because the deductions from the model in *Simulated Worlds* do not reveal much of interest regarding the real world or the recent social science literature, it seems reasonable to expect them to contain explanatory statements concerning relationships between variables. One would hope that these statements will take the form of testable hypotheses or propositions serving the basis for the derivation of hypotheses.

The author gets at model suggested relationships between variables in chapter 5 by correlating the generated values (or transformations of them) for three national attributes (size, development and accountability) with the generated values for 14 national behavior variables (e.g., defense spending). Bremer compares the correlations for the 42 relationships obtained using the model-produced data with those obtained by using INS and real-world data. This constitutes an empirical test of the relationships suggested by the computer model and adequate fits are reported for some of the relationships. Bremer also gets at model-generated relationships between variables by altering selected initial variable and parameter values and determining whether there are changes in the generated values for arbitrarily selected variables. This suggestive sensitivity analysis is reported in chapter 4; this is the most interesting chapter in the book and should have been given greater scope. The unit of analysis is the nation-state and the focus is on the relationship between historical and policy factors and the levels of productive resources, military capability, political stability and international conflict. Little that is new is addressed in chapters 4 and 5 because the literature already contains discussions and justifications for most, if not all, of the suggested relationships between variables. One expects that the model will provide explanations for these various relationships. Unfortunately because of the complexity of the model and ambiguities in presentation, we cannot be sure why these relationships were generated.

An earlier draft of this manuscript received the 1972 Helen Dwight Reid Award from the American Political Science Association for the best dissertation in the field of international relations, law and politics. I am, however, disappointed in the book. I agree with the goals and approach, but not with the execution of the research. There are very few substantive or methodological contributions in *Simulated Worlds.*

FRANCIS W. HOOLE

Indiana University

The Anarchical Society: A Study of Order in World Politics. By Hedley Bull. (New York: Columbia University Press, 1977. Pp. xv + 335. $20.00.)

This is a reasoned, penetrating, stimulating, persuasive, and easily read analysis of the nature and function of order within the society of states in contemporary world politics. It is concerned less with describing and proving the anarchical character of the international community than with probing such basic questions as: What is order in world politics? How is order maintained in the contemporary state system? What alternative routes to world order are desirable and feasible? Not regarded as an end in itself, or as the only or necessarily an overriding value, order is defined simply as "an actual or possible situation or state of affairs" (p. xii).

The author, Montague Burton Professor of International Politics at Oxford, concedes that this volume is not the product of refined theoretical technique or of any particularly recondite historical research. Nevertheless, he interweaves threads of history, philosophy, politics, and pragmatic inquiry and thinking into a variegated fabric of concepts, principles, interpretations, and optional possibilities. Concerned with literary longevity, he confines himself to enduring matters of structure and function and avoids contemporary substantive issues. His objective is to provide understanding, not to advocate. He makes it clear that he is not prescribing solutions or canvassing the merits of any particular vision of world order.

The volume consists of three parts. The first poses the subject and its parameters, and contains chapters on the concept of order, its existence, its maintenance, and its relationship with justice in world politics. Juxtaposing order and justice, the author contends that Western states are primarily interested in preserving order, while the Third World is largely concerned with what he terms "just change," even at the cost of disorder. He believes that justice is achievable only in the context of order, that the two are compatible, and that international society is not inhospitable to notions of justice even though it may often violate them.

Part 2 contains chapters on the methods of promoting order in world politics: balance of power (including balance of terror), international law, diplomacy, war, and great power stabilization. The author deliberately excludes institutionalization by means of international organization (the United Nations system), although he broaches it in dealing with multilateral diplomacy. He evidences awareness of recent developments, such as changes in the scope and application of international law, "the new diplomacy," nuclear weaponry, and polycentrism. Some readers will find certain views—that the inequality of state simplifies the pattern of interrelations, that the exploitation of preponderance imparts direction to the affairs of international society, and that order sustained by the great powers enjoys general support throughout the global community—especially challenging if not provocative.

The last part focuses on alternative paths to world order. In it the author examines possible shifts in the state system, such as a disarmed world, solidarity of community, ideological homogeneity, and widespread nuclear proliferation, only the last of which he conceives as probable. He also reviews more fundamental alternatives to the states system, such as the demise of sovereign states or of the aggregate system, world government, a new medievalism (involving a network of overlapping authorities and multiple loyalties), and unspecified non-historical alternatives. He regards each of these as theoretically conceivable, but as unlikely and failing to guarantee greater order.

In his chapters on the decline, obsolescence, and reform of the state system—the heart of his analysis—the author argues that it is bound to continue in the decades ahead and that the key problem is how to achieve the best reform. If it is regarded as obsolete or dysfunctional, this does not require its replacement, but suggests the mandate for states to extend their sense of common interests, rules, and institutions to better meet their needs. He examines and assesses such reformative possibilities as a great power concert (Kissinger model), global centralism (radical salvationist model), extended regionalism (Third World model), and revolution (Marxist model), and finds each to be visionary, unproven, ambivalent, and/or unenforceable.

He is convinced that the best prospect is to keep the state system and assure that it remains viable by "maintaining and extending the consensus about common interests and values that provides the foundation of its common rules and institutions. . . ." Such consensus, in turn, must include "a sense of common interests among the great powers, sufficient to enable them to collaborate in relation to goals of minimum world order" and take into account the requirements of Third World countries "who represent a majority of states and of the world's population. . ." (p. 315). He concludes, therefore, with a precept rather than a prescription and he cautions: "It is better to recognize that we are in darkness than to pretend that we can see the light" (p. 320).

ELMER PLISCHKE

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Social Change in the Twentieth Century. By Daniel Chirot. (New York: Harcourt Brace Jovanovich, 1977. Pp. xii + 273. $5.95, paper.)

Daniel Chirot has undertaken a most ambitious venture into the sociology of world politics. Critical of existing theories, and of programs of political action derived from such theories, he has made a new attempt to discover the politically significant relationships between internal and international social, economic and political change.

Chirot begins by asking why the benign "liberal" interpretations of underdevelopment by Millikan and Rostow, Almond, Deutsch, and

Parsons, expressed in the foreign aid policies of the 1960s, produced failure and disillusion. Dissatisfied with the neo-Leninist answer that underdevelopment, as a product of willful imperialist exploitation, can only be remedied by revolution, Chirot develops a core-periphery analysis of twentieth-century world politics partly based on the ideas of Immanual Wallerstein. "Core" societies—relatively rich, powerful, industrialized, economically diversified nation-states—by investing and trading for their own advantage, have created "peripheral" societies which are poorly integrated, weak, subject to external control, economically overspecialized. Some countries have attempted to escape their disadvantageous peripheral condition: Meiji Japan and the repressive, industrializing Russia of Witte, Plehve and Pobedonostsev were among the earliest of these "semi-peripheral" societies.

A semi-peripheral society struggling to achieve core status faces significant resistance from the established core societies, which have some interest in keeping the poor countries dependent and specialized in certain types of production: to some organized and powerful groups in the core countries this interest is vital and consciously pursued; to the core societies as a whole, the "interest" is marginal, reflected mainly in foreign investments which, reasonably aiming for secure profits, tend incidentally to perpetuate dependence.

"Liberal" development strategies calling for free trade, increased private foreign investment, and expanded economic and technical assistance, actually promote increased dependency, and inadvertently sharpen class conflicts within non-core societies and between core and non-core. Neo-Leninist strategies, accepting such conflicts as inevitable and indeed useful, deliberately exacerbate them. Chirot, convinced that the real net advantages which core societies obtain from exploiting the periphery are small, and that economic growth that reduces social stratification can alleviate rather than sharpen conflict, suggests an alternative strategic model for development.

Chirot's program for semi-peripheral societies aspiring to core status is neo-Stalinist rather than neo-Leninist: nationalism, isolationism, statism, forced-draft modernization. Adopt core ideas of economic, scientific and technological rationality and growth; reject free-market and democratic ideologies in favor of puritanical absolutist egalitarianism. The state is to be strengthened, regional and ethnic groups assimilated to a national culture. Investment must be maximized at the expense of elite, middle-class, peasant and working class consumption; the inevitable resistance to this policy must be repressed. Isolation is essential: foreign luxury goods must be prevented from draining away the economic surplus, which is required for investment; core country manufactures cannot be permitted to stifle infant industries; core culture-products must be excluded to avoid arounsing individual expectations that cannot be met. Expensive and risky foreign adventures are to be avoided. The semi-peripheral society that aspires to core status must pay the price: closure, rigidity, autarky and bureaucratization.

Core countries would do well to resign themselves to the loss of such investment outlets, cheap raw-materials sources, luxury-goods markets, and companionable regimes as they still find among the non-core societies. Resignation will become easier if the core societies realize that the profits, services and gratifications they now derive from the periphery are useful, but in no sense necessary: there are even ways, if need be, of doing without energy and raw material imports from the periphery; learning to do without is indeed the core's basic task.

Should these imperatives be followed, Chirot foresees a long epoch of austerity, isolation, and danger for core and non-core societies alike; but then, perhaps in the next century, there may evolve a more open world order, more peaceful too because balanced and prosperous, composed rather of bureaucratic than of democratic societies, far from ideal, but still quite tolerable.

Chirot's arguments in support of his theory and program are extensive, careful, empirical. At this moment Chirot's interpretation of recent international history seems to fit rather better than either developmental liberalism or neo-Leninism. Though his work will hardly be welcomed, it is too substantial to be swiftly or easily refuted. Perhaps this very provocative little volume will eventually provoke the creation of a social theory and political program as plausible as Chirot's, and more appealing. Whatever may come of it, *Social Change in the Twentieth Century* surely constitutes a major contribution to the attempt to create a theory of world politics useful both for understanding the history of international affairs and for changing it.

DAVID WILKINSON

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Southeast Asia in International Politics, 1941–1956. By Evelyn Colbert. (Ithaca: Cornell University Press, 1977. Pp. 372. $17.50.)

Evelyn Colbert, a Department of State and Central Intelligence Agency analyst, presents a detailed, comprehensive survey of Southeast Asia's international politics from the opening of World War II to Vietnam's failure to hold elections on reunification in 1956. Specialists and general readers alike will find her book a lucid, useful synopsis of all major international events in this 15-year period.

The book emphasizes the importance of colonialism in the formation of the modern Southeast Asian international system. The section on decolonization accounts for over a third of the book, and the perceptions held by Southeast Asian leaders with respect to most issues are presented as deeply influenced by this experience. During World War II, Colbert points out, Southeast Asians could see little difference between the Western contestants, and after the war they rejected the victors' view that nationalists who had collaborated with Japan were "Quislings." Similarly, Southeast Asians could see little sense in joining a European-American crusade against communism so long as their first enemy remained European colonialism. These perceptions changed, according to the author, following independence, when domestic insurgencies and, until 1950, Sino-Soviet vilification of "bourgeois nationalists" in accordance with the two-camp thesis, suggested neutralism or varying degrees of tilt toward the West would serve Southeast Asian interests best.

Major topics covered in the rest of the book include the impact on the region's international politics of China's regeneration, the Korean War, the doctrines of neutralism and peaceful coexistence, the Indochina War and the Geneva Settlement, SEATO, and the Bandung Conference. Although the author does not make the point explicitly, it is reasonable to surmise on the basis of her choice of topics and the book's title that she assumes the agenda for international politics in Southeast Asia to have been determined mainly by forces outside the region, the policies of Southeast Asian governments to have been largely reactive, and intraregional politics to have been much less significant than world politics in shaping the region's fate.

Relying on judicious use of secondary and open documentary sources rather than on hitherto unexploited materials, Colbert does not strive for original interpretation so much as for dispassionate observation. The result often is to remind us, with unintended irony, of facts which neglect and political debate have obscured. For example, she shows that economic considerations first entered American policy debates on Southeast Asia as justifications to oppose colonialism and support independence. The Korean War, she argues, did not stimulate the U.S. to oppose communism by force on the Southeast Asian mainland but rather, for fear of arousing "public opposition to 'another Korea,'" to advocate strengthening "indigenous willingness and ability to resist" (p. 161). The American military, she observes, opposed intervention in Indochina because they held Indochina to be "devoid of decisive military objectives" and intervention a dangerous diversion of limited U.S. capabilities (p. 250). And not long afterward, at Bandung, Asian nations were divided on cold war issues but many strongly criticized "Soviet imperialism," possibly encouraging Chinese leaders later to take the position they now press so vigorously.

Focusing on the actions of governments in diplomatic contexts, on official reasons for official positions, Colbert's analysis gives much more attention to finished policies than to the processes by which these policies evolved. While this focus reflects the limitations of sources and the need for analytical economy, it leaves the impression that governments, Southeast Asian ones in particular, were unitary entities and that leaders were committed to their nations' interests without concern for personal fate or fortune. The analysis could have been strengthened by recognition of the extent to which many Southeast Asian elites were inclined by social class, education, and opportunities for personal aggrandizement to align with the West, and that this inclination tended to be stronger in proportion to their isolation from their own peoples. Colbert obviously is sensitive to the domestic sources of foreign policy in one government, for she makes excellent use of *The Pentagon Papers* to show how bureaucratic interests and congressional politics conspired to deprive American policy of coherence and enlightenment. It is a pity that other governments were not, or could not be, subjected to similar scrutiny.

The book's major shortcoming is that no general conclusions emerge from the carefully assembled detail. The reader will note the striking parallel between Soviet and American attitudes toward the region following World War II, for example, but will not find any discussion of the significance of this fact. In this particular instance, the superpowers subordinated policy toward Southeast Asia to their interests in Europe, thinking Southeast Asia had no strategic importance in itself. The results of this priority, however, were to fuel the French drive for greater autonomy within the Western alliance while drawing both the Soviet Union and the United States deeper into Southeast Asian affairs regardless of their judgment as to the region's intrinsic importance. Colbert wisely avoids the kind of empty abstractions that may come to mind when searching for patterns in her impressively thorough yet concise presentation, but at the same time readers with a bent for generalization or theory might wish she had made greater effort to synthesize.

WILLIAM S. TURLEY

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Illusions of Choice: The F-111 and the Problem of Weapons Acquisition Reform. By Robert F. Coulam. (Princeton, N.J.: Princeton University Press, 1977. Pp. xiii + 432. $21.50.)

The acquisition of weapons systems is expensive business. Moreover, American experience with systems such as the C-5A transport, the Cheyenne helicopter, the F-14 tactical fighter aircraft, and the MBT-70 tank suggest that it is a business in which initial estimates of cost and performance can be notoriously unreliable.

One of the most costly and controversial systems of recent decades has been the TFX (tactical-fighter-experimental), later designated the F-111. At Secretary of Defense Robert McNamara's insistence, a tactical fighter was to be developed which would serve air force needs for air superiority and interdiction, as well as the navy's needs for close support of the fleet. An earlier study (Robert J. Art, *The TFX Decision: McNamara and the Military*, Boston: Little, Brown, 1968) provided an analysis of McNamara's selection of General Dynamics as the prime contractor for the TFX, despite an unanimous recommendation by military officials that the contract be awarded to Boeing. Robert Coulam's study focuses on the continuation, for several years after the initial contract decision, of development and production of the F-111, even though it was proving to be expensive far beyond initial estimates and subject to a rash of performance failures.

Problems encountered with the air force version of the F-111 and then with the navy version provide distinctive but interrelated insights into decision-making behavior. The Tactical Air Command (TAC) had postulated in the late 1950s that a successor aircraft to the F-105 must be effective in a nuclear interdiction mission. When McNamara took over as Secretary of Defense, he was determined to move beyond fixation with a "massive retaliation" strategy and to develop limited war capabilities. However, Coulam demonstrates that McNamara's eagerness to push a joint air force-navy tactical fighter program led him to accept the TAC definition of requirements, largely oblivious to trade-offs that were implicit in the decision. Moreover, the deference of McNamara's Systems Analysis staff to the Department of Defense Research and Engineering during the development phase of weapons acquisition biased the staff analysis of the TFX design in favor of sophisticated avionics. But once the development decision was made, even though McNamara was becoming more aware of the nuclear-mission emphasis of the TRX decision, it was too late for substantial modifications.

The sequence of events which led from a reluctant navy commitment to the F-111B, to cancellation of the contract in 1968, provides an even more striking illustration of the politics of the weapons acquisition process. The navy, dubious from the start about the feasibility of developing a plane which could meet both air force and navy needs, found itself neglected in design specifications. Navy personnel who conducted flight tests found deficiencies that merely confirmed their suspicious about the unsuitability of the F-111B. Cancellation occurred within a few months after McNamara's departure from the Defense Department, in a milieu in which navy opposition to the F-111B had been joined by powerful members of Congress.

The Coulam study is interesting as an empirical examination of the utility of a theoretical paradigm of decision making. Glenn Paige's application (*The Korean Decision*, New York: Free Press, 1968) of the framework that had been developed by Richard C. Snyder et al. (*Decision-Making as an Approach to the Study of International Politics*, Foreign Policy Analysis Series No. 3, Princeton University, 1954) was a notable earlier work of this genre. Similarly, Coulam applies directly a framework developed by his mentor, John D. Steinbruner (*The Cybernetic Theory of Decision*, Princeton University Press, 1974). However, Paige had taken a "skimpy stockpile of theoretical tools" (in Snyder's words) and sharpened them at least to the extent of developing empirically testable propositions that were missing from the original Snyder design. Coulam, in contrast, suggests neither modifications nor additions to the Steinbruner paradigm.

Moreover, the sheer volume of technical details (as exemplified in a chapter on "Engine-Inlet Compatibility Problems") tends to blur rather than to sharpen the theoretical framework of the analysis. Nevertheless, this careful study enlarges our understanding of decision-making behavior in an important policy arena. Coulam's depressing but instructive conclusion is that the F-111 experience was typical of, rather than a deviation from, the pattern of waste and inefficiency in programs of weapons acquisition—programs that seem to defy reform despite widespread recognition of the magnitude of the problem.

JOHN P. LOVELL

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Egypt in the Arab World: The Elements of Foreign Policy. By A. I. Dawisha. (New York: Halsted Press, John Wiley, 1976. Pp. xv + 234. $24.50.)

In this relatively short but useful book, A. I. Dawisha employs a somewhat different approach in discussing and analyzing Egypt's relations with other Arab states since 1952.

In part 1, the author describes the development of Egypt's foreign relations during the Nasser period, using the method of historical analysis. In a much longer and more methodologically oriented part 2, the author first establishes a simplified input-output model which attempts to identify the elements of Egypt's foreign policy and their classification under a series of categories. He then discusses in separate chapters each of these categories—the capabilities and constraints on Egypt's foreign policy, the institutions and processes of Egyptian policy making, the values and images of the decision makers, the political and economic objectives pursued by the policy makers, and the instruments used to achieve these objectives. In a final chapter, he brings his analysis up to date by applying his model to the Sadat period.

Because of the scarcity of good studies of Egypt's foreign policy, this scholarly book should be of some interest and value to students and general readers. Since, on the whole, the factual materials presented are fairly well known to most scholars and specialists dealing with the Middle East, they will be primarily interested in not only the methodological aspects but also the author's analyses—such as those related to the roles of the defense establishment and the foreign ministry in the process of making foreign policy and to the fluctuations in Egypt's foreign policy goals between the "maximum objective" ("comprehensive unity"), the "intermediate objective" ("revolutionary change"), and the "minimum objective" ("Arab solidarity").

Dawisha contends that between 1955 and 1967 propaganda was the "primary instrument of foreign policy" (p. 171). Although he perceptively notes that Egyptian propaganda was primarily aimed at crystallizing and encouraging "already existing attitudes and beliefs which proved to be susceptible and sympathetic to Cairo's message" (p. 14), at times he seems to exaggerate the degree of influence of this propaganda on certain actual events. For example, on page 171, merely on the basis of coincidence and without any solid proof, he appears to give full credit to a week of intensive Cairo radio propaganda for Iraq's withdrawal from the Baghdad Pact on March 24, 1959. Meanwhile, there is considerable, strong evidence (see, for example, *Iraq Under General Nuri* by Waldemar J. Gallman, pp. 85 ff.) that the Kassim government had decided soon after seizing power in July 1958 to withdraw at some convenient time in the future. Thus, in some places in the book, Dawisha should have made a clearer distinction between situations where propaganda actually played a major and direct role in influencing events and other situations where propaganda merely reinforced an existing inclination to take certain actions without being primarily responsible for those actions.

Largely, although not wholly, because the author tries to cover so much ground in such a relatively short book, he ignores some rather important events and developments while either oversimplifying or inadequately explaining others. For example, while he discusses the 1953 Anglo-Egyptian agreement on the Sudan, the author ignores the important 1954 Anglo-Egyptian Suez agreement and its significant consequences. Moreover, throughout the book the author generally gives the impression that Nasser was always and in every respect vehemently anti-Western. He completely overlooks the pro-Western views of Nasser in the period between 1952 and 1954. In fact, no reference is made to strongly pro-Western and anti-Soviet statements made by Nasser, such as those made in August and September, 1954. For example, on September 2 (see the *New York Times*, 3 September 1954), Nasser said that "there seems to be no doubt that Egypt stands in every respect with the West"; the "only possible danger to the Middle East on a global scale was an invasion by the Soviet Union"; and Egypt could be prepared to join a Western alliance after the Egyptians had had "a period of complete independence." Nasser became "anti-

Western"—more accurately anti-Western policies and actions and not anti-Western civilization, culture, and the like—only after and as a consequence of the Baghdad Pact, the Israeli Gaza attack on February 28, 1955, growing personal hostility of Dulles and Eden to Nasser, and other developments, not all of which are adequately explained.

On the whole, this well-written, well-organized, and well-documented book is a most valuable and welcome addition to the all too few worthwhile studies on Egyptian foreign relations since 1952.

FRED J. KHOURI

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War Clouds on the Horn of Africa: A Crisis for Détente. By Tom J. Farer. (New York and Washington: Carnegie Endowment for International Peace, 1976. Pp. xi + 157. $3.75, paper.)

Once an arcane corner of African studies inhabited by a handful of academic specialists, mostly historians, the Horn has at last become a strategic magnet attracting the concern of the media, policy planners and even the public. There is, indeed, something for everyone: a superpower rivalry over an area bordering on the Indian ocean and the Red Sea oil routes, a confrontation between Coptic Christianity and militant Islam. The Horn, a clutch of countries on the fault line between the black south and the Arab north, is not only an arena for Middle East conflict, but also the site of some of the oldest and most inbractable ethnic quarrels. For 16 years there has been the classical rural insurgency in the northern Ethiopian province of Eritrea and, since 1960, the rarely muted ambition in Mogadishv for a Greater Somalia that has led to clashes in Djibuti, guerrilla raids into Kenya, and presently conventional war in Ethiopian Ogaden—or liberated Western Somalia. There have been coups and assassinations, revolution proclaimed, and gunfire in the streets. All this might have remained of interest to the specialists. The main rivals in the Horn, Somalia and Ethiopia, are impoverished, isolated and lacking in resources; they are subsistence states with fragile governments and dim prospects. In Ethiopia, outside of a few towns, there are few square buildings, over 90 percent of the people live beyond roads, and nearly every vile disease known to man flourishes. Somalia, located along a bleak coast, even lacks a written language at independence. The Horn, however, has not been left in splendid isolation but become the focus of major power interest. Thus the appearance of Tom Farer's *War Clouds on the Horn of Africa* is aptly subtitled "A Crisis for Détente." The Horn is important and important to those who know little of the area. After Farer's superb study, this will no longer be an excuse—at least in Washington—for in 157 pages he covers in detail the nature of the Horn, past and present, and the implications for the future.

Although Farer's analysis was completed in 1976 before the Russians changed sides, backing Ethiopia and alienating Somalia, thus invalidating some of Farer's conclusions, the crucial point in the book is that although the Horn does exist in a geopolitical context of very considerable interest, especially in a fragile era of détente, the quarrels are parochial, complex and long-lived. In his final sentences of *The Margin of Policy,* Farer indicates the scope of the problem: "If Somalis and Ethiopians cling to their historic enmity, the odds favor war on the Horn of Africa. And if war comes, the odds will even more heavily favor the further unraveling of détente" (p. 151). And his last word is "The Horn is not an especially hospitable setting for human habitation. But as a venue for confrontation by proxy, it now shows real promise" (p. 153). After reading *War Clouds on the Horn of Africa,* no one responsible for those margins of policy can bungle into crisis, no more Angolas or Cyprus. Farer has written a fine book, a timely book, a valuable book, academically sound, based on personal exposure to the reality of the Horn, sensible on the problems and prospects that the Horn presents for policy makers. The Carnegie Endowment for International Peace should be delighted that the project to identify problems before they have occurred has in this case borne such splendid fruit. Certainly Farer and others would have wished that the war clouds had blown away; but at least now amid the storm, we know a great deal more about the anatomy of this very special crisis and the margins of policy.

J. BOWYER BELL

Columbia University

The Scandinavian Option: Opportunities and Opportunity Costs in Postwar Scandinavian Foreign Policies. By Barbara G. Haskel. (Oslo: Universitetsforlaget, and New York: Columbia University Press, 1976. Pp. 266. $14.00.)

Foreign observers of Scandinavian politics, who are at least initially impressed by the institutional similarities of the three Scandinavian states, are often puzzled by the limited achievements of successive efforts to promote regional political and economic integration. In contrast to the fitful but significant achievements of the European Communities during the past quarter century, the accomplishments of the Scandinavian nations (Denmark, Sweden, and Norway) or, more broadly, of the Nordic countries (the above plus Finland and Iceland) have been much more modest. One must in fact go back to late medieval history, the Kalmar Union (1397–1520), to find all five countries under one ruler. The central question of this study is broad: "why, given the seemingly propitious wealth of shared characteristics, did these three countries form so few and so weak joint arrangements?" (p. 11) The ensuing analysis contrasts the large number of Nordic "contacts" with the very limited number of Nordic "commitments."

Haskel's investigation focuses upon the three main efforts at developing Nordic regional commitments in the postwar period: the proposed Scandinavian defense pact of 1948–49, the various Scandinavian common market plans of 1947–1959, and finally the smaller pragmatic efforts of the Nordic parliamentary committees which gave rise to the Nordic Council and specific regional agreements during the 1950s. Although Haskel does not intend her study to be an encyclopedic review of Nordic regional affairs in the postwar era, she identifies and places in perspective nearly all significant events of the period. This, given the extended nature of Nordic and Scandinavian negotiations, is a meaningful achievement in a short study.

Haskel attempts more than historical analysis; she has applied techniques of economic costing analysis in a test of the reasons for or against the eventual success of a regional proposal. The costs and benefits expected from each alternative, and outside events that changed the calculus of decision.

The effort to form a Scandinavian defense alliance in 1948–49 remains one of the more dramatic attempts at regional integration. As the cold war became apparent to the political leaders of the three Scandinavian democracies, they sought alternatives to the isolated neutrality of the late 1930s, which had failed to prevent the German occupation of Denmark and Norway. Sweden had avoided direct participation in World War II, though transit concessions were made to Germany during the early years of the war and assistance was given the Norwegian and Danish Resistance toward the end of the war. Hence perspectives differed at the outset of the Scandinavian defense negotiations with Norway wanting assurances of Western support for a regional pact both in rearmament and in case of attack. Sweden insisted, however, that a Scandinavian pact to be free of any commitments to one or more of the great powers. Denmark's position was more flexible, and her leaders sought to mediate the Swedish-Norwegian differences. Norwegian leaders felt that American assistance in rearmament and in the event of attack were, after November 1948, contingent upon membership in the broader Atlantic Pact, and given the priority Norwegians attacked to deterring the defeating a potential attacker (as had not happened in 1940), the Atlantic alternative seemed preferable.

Various efforts to remove barriers to trade and economic activity constitute the second issue for analysis. In this and the following case of pragmatic efforts to expand Scandinavian and Nordic cooperation in a variety of functional areas, Haskel again does more than summarize the complex and extended negotiations. By applying the costing model of decision making, one gains a more realistic and comprehensive perspective on the trade-offs between regional integration and national policy priorities. It is interesting that, as in the case of the attempted regional defense pact, the Scandinavian option was weakened by both domestic politics and by larger continental developments.

This is then a useful study, concisely and gracefully written. It adds significantly to the English-language literature on modern Scandinavian politics and gives students of political integration invaluable theoretical and empirical information.

ERIC S. EINHORN

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China and Japan: A New Balance of Power. Edited by Donald C. Hellmann. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xxi + 305. $16.95.)

This very good book, directed to policy makers and business people, also contains much meat for college teachers and students in political science, economics, and history. Its detailed examination of the "critical choices for Americans" focuses on some real problems of the next decade or so. Transnational relations receive due attention, but more often implicitly than explicitly. The overall tone is broadly optimistic and blessedly free of jargon and Armageddon language and thought. There was clearly no attempt to impose any model of thinking about the issues; indeed, the editor indicates his partial disagreement with some of the contributors.

Editor Hellmann's thoughtful introduction lays out some crucial issues and the need to deal with China and Japan on both global and regional levels. In various ways, all the authors deal with this global/regional issue, though not always to the reader's satisfaction.

An unabashed believer in *Realpolitik*, Hellmann barely disguises his apparent contempt for Japan's allegedly Alice-in-Wonderland approach to foreign affairs—in particular its refusal to accept greater security responsibilities. But should Japan "go nuclear?" "From one perspective" (Hellmann's?) this decision is "both unlikely and unwise" because it "is absurd to suggest that an island nation" as compressed as Japan "can devise an appropriate defense or nuclear counterforce to assure national security." It will not be politicians, but rather the "drift of world events" beyond Japan's "capacity to control" which may push Japan down the nuclear road. Thus, the critical choice for both Americans and Japanese is to impede this drift.

A less gloomy Chalmers Johnson provides an excellent case study of Japan's reaction to the oil crisis, and concludes from this and other artfully stated points that Japan is "not 'going nuclear' but rather 'going internationalist' in what may prove to be one of the most important and beneficial examples in modern history of an advanced industrial nation exercising great economic power but little military power." The core of Johnson's essay rests on an interesting discussion of the implications of five problems or "barriers" facing Japan—the environment, the work force, resources, technology, and markets. Johnson's recommendations stress interdependence in general and the American connection in particular.

Gary Saxonhouse and Hugh Patrick deal with Japan's economic fortunes in the post-Bretton Woods and post-oil crisis world. Perhaps better than any other short piece, their richly textured essay explains, beyond bland generalities, why Japan is a global economic superpower and, more important, pinpoints the potential problems and opportunities associated with that status. Their analysis carries far beyond the dubious truism that Japan's foreign policy is essentially a never-ending search for resources and markets.

Dwight Perkins' essay about constraints on Chinese foreign policy centers on the economy and how this "in turn influences China's military capacities and its economic programs abroad." Agriculture, the main drag on the economy, continues to be an unresolved problem. The means to alleviate this problem are quite limited, but in some manner more capital must be generated. Perkins does not accept the fashionable notion that China will become an "instant Saudi Arabia," but he does argue that greater Chinese oil exports might have a worldwide "stabilizing effect." In contrast, a continuing agriculture problem could bring China more actively into the world agricultural market, which in turn might "accentuate current shortages."

Perkins argues that China cannot easily project its substantial military power beyond its borders, mainly because of the strong nationalism of nearby countries, the increasing ability of these nations to manage their own affairs, and the booming economies of several of them. China's limited ability roughly parallels U.S. limitations, and suggests a kind of parallelism of interests. These points support Perkins' most specific recommendation: "it should be the goal of the United States to create conditions that will have the long-run effect of reducing the influence of American power in Asia, not by substituting some other power, but by decreasing the influence of all the major powers."

Perkins also deals with the nettlesome disarmament issue, and suggests that Peking's current "disinterest or outright hostility" may shift when China's relative military strength grows. If and when that occurs, "there is every reason to believe that the Chinese will worry less about freezing the status quo and more about the dangers of a world full of nuclear weapons."

Thomas Robinson's discussion of the political-strategic aspects of Chinese foreign policy contains many striking observations, yet the piece is so structurally complex that one supposes that busy decision makers would soon lose patience.

Michel Oksenberg's chapter should draw great attention because, aside from its excellence, Oksenberg has become the senior China expert in the National Security Council. With a subtle sense of history, Oksenberg spells out the complexities of future Sino-American ties. Yet, these same complexities offer a variety of opportunities to bolster U.S.-China relations. His multilevel analysis goes far beyond the misty-eyed notion that formal "normalization" of relations is an end product. Rather, substantial improvements will involve a "laborious process of flexibly piecing together" a mutually beneficial relationship.

DONALD W. KLEIN

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American Democracy and World Power. By Robert A. Isaak. (New York: St. Martin's Press, 1977. Pp. viii + 216. $12.95, cloth; $4.95, paper.)

This short volume is an extended essay which wrestles thoughtfully with the essential dilemma stated by Robert Isaak at the outset: "Americans are torn between their democratic values and their conservative empire" (p. 1). What follows is a spotty but not uninteresting contribution to what should be a far sharper public debate on how to adapt the American system to changed circumstances.

As the author goes to the mat with "the American predicament" (the title of his first chapter), he moves through a series of categories growing out of his philosophical bent—liberalism, conservatism, pragmatism, materialism, welfare, privatisitic versus civic obligations, and the rest. At root he is striving to give yet another answer to the question of just what is a liberal foreign policy. In trying to get beneath the easy generalizations, he comes to grips in his chapter on "American values and the American image" with the struggle between "ideological, bourgeois realism" versus "mystical protestant idealism." He stumbles, at least in this reviewer's opinion, in crediting "the moral perceptions of the hippies" for progress toward a new popular moral awareness in the United States (p. 25). Perhaps because he was a recent quasi-participant, he feels that the counterculture "may have bequeathed Americans a set of value priorities and a sense of community awareness and commitment that can serve to restore American morale at home in the future" (p. 149). I don't think we know yet what it is that is reshaping American value priorities. I suspect that the primary source of change happens outside the campuses and coffeehouses where the counterculture was a dominant force. Surely American political life is more accurately described as finding its values in the mysterious but redemptive socio-political faith of the eternally optimistic, chronically put-upon, "straight" American middle-class majority, most of whom wouldn't give you a fig for the visible values of the hippies.

I would take much more seriously the central political issue posed by Isaak, namely, how to "redirect this creative, restless energy and productivity, this aggressively rugged individualism, toward social purposes" (p. 28). This is a toweringly relevant question for determining how successful American society will be in the decades ahead. Isaak's prescription is "an open-ended political system of 'democratic elitism' domestically" and "democratization of regimes internationally" (p. 11). If this sounds Wilsonian, it lacks the blind optimism of the Progressive-cum-League of Nations crusade in its vision of a Sisyphean America pushing its liberal idealistic stone uphill in a refractory world. In the end, the recommendation is for a decoupling of political democracy and social and economic justice from the international pursuit of capitalist profits. If the U.S. intervenes abroad, it should be to encourage democracy, not capitalism, and to foster the economic growth of others, not American profits (p. 135).

In addition, there are chapters on forms of government and on policies toward the Third World, criticisms of Henry Kissinger's preoccupation with trilateralism, and excursions into the educational process.

A weakness of this book lies in its replowing some familiar terrain, its rediscovery of some much-remarked dilemmas, and its generalized (humanitarian-liberal-democratic) solutions for each sector of the problem at which the author takes a swing. The reader is constantly tripped up by thickets of quotations and citations from far too many people. (Why on earth is Otto von Hapsburg the authority for a statement [p. 111] that the Soviet Union is a "paper tiger"?!)

I hope that the author in his next book has the confidence to get rid of nine-tenths of the people he quotes to bolster his own line of reasoning, and concentrates instead on giving us a more programmatically satisfactory exposition of the problems he correctly and thoughtfully confronts. He ends on a weirdly negative note. But then, neither he nor his readers yet knows what of the recent era of convulsive change is going to endure, and what was just the scum on the wave of the future.

LINCOLN P. BLOOMFIELD

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Security in the Nuclear Age: Developing U.S. Strategic Arms Policy. By Jerome H. Kahan. (Washington: The Brookings Institution, 1975. Pp. ix + 361. $12.50, cloth; $5.50, paper.)

The accumulation over the last three decades by both the United States and the Soviet Union of thousands of nuclear weapons and the means for delivering them at intercontinental ranges has, not surprisingly, stimulated a wealth of scholarly and journalistic accounts on virtually all facets of the strategic arms competition. The stability of the military balance has been and continues to be analyzed in great detail. The strategic arms limitation agreements, the ongoing negotiations, and formulas for prospective agreements have all been the subject of considerable commentary and debate. An impressive number of case studies has been generated attempting to describe the web of bureaucratic and political forces that have strongly influenced the deployment of particular U.S. strategic weapons systems. And a variety of broad-gauged efforts have grappled with the elusive problem of assessing the political utility of the strategic forces. Coupled with the enormous amount of information on these matters placed in the public domain by the Department of Defense and the U.S. Arms Control and Disarmament Agency, the reports and hearings of congressional committees and other organizations affiliated with the Congress, the output of a number of government-related, independent, and university-based research institutions, and the detailed accounts provided regularly in the press and the trade journals, the quantity of material on this subject that is available to the interested reader is truly staggering.

What is surprising, therefore, is that this literary torrent has failed to produce a single comprehensive historical analysis of the development of the Soviet and American nuclear arsenals—with the possible exception of George Quester's *Nuclear Diplomacy* (1970), which ends at the dawn of the Nixon administration and consequently omits much that is central to the contemporary Soviet-American strategic relationship. Jerome Kahan's *Security in the Nuclear Age* attempts to fill this gap, at least in part, by providing a detailed account of the development of U.S. strategic arms policy from 1953 through 1974. It offers as well a lengthy assessment of future strategic policy issues.

Written when Kahan was a senior fellow in the Brookings Institution's Foreign Policy Studies program—he is now a member of the Policy Planning Staff of the Department of State—the book, according to the foreword, reflects not only the author's research and analyses, but also the discussions of a study group sponsored jointly by Brookings and the Carnegie Endowment for International Peace. The study group included many luminaries in the field: Harold Brown, Philip J. Farley, John S. Foster, Jr., Morton Halperin, Franklin A. Lindsay, George W. Rathjens, Herbert Scoville, Jr., Gerard C. Smith and Herbert F. York, to name a few.

With the assistance of this formidable supporting cast, Kahan has produced a strong work. In a clear, organized, documented and balanced presentation that has become the hallmark of Brookings' publications in foreign and defense policy, the author systematically reviews the evolution of U.S. strategic policy from the heyday of the posture of massive retaliation, through the McNamara period and the development of the doctrine of assured destruction, to the Nixon-Kissinger efforts in dealing with strategic parity and the Strategic Arms Limitation Talks (SALT). There is a wealth of historical detail presented in this part of the book and it will surely be a helpful guide, particularly to new students of U.S. strategic policy. Moreover, the second part, entitled "The Search for Stability," enumerates many of the issues that have been at the core of the American strategic arms debate in the 1970s: notably, the requirements to satisfy the assured destruction criterion and various proposed alternatives to it; the search for flexible strategic options; the problems plaguing negotiated strategic arms limitations; and the desirability of maintaining a stable deterrent.

Nonetheless, the shortcomings of the work are not trivial. There is, first of all, an apolitical quality to the analysis that leaves the reader only vaguely aware of the intense domestic political and bureaucratic considerations that have shaped and continue to influence U.S. strategic arms policy. In part, this quality is a reflection of the "on-the-one-hand, on-the-other-hand" style of much of the analysis which tends to convey such a balanced account that the perennial tugging and hauling which is characteristic of the U.S. strategic arms debate fails to be conveyed adequately. And, in part, it is because the mode of analysis emphasizes rational strategic arguments and devotes too little attention to presidential ambition, interservice rivalry and other substantive nonstrategic determinants of U.S. policy. Second, there are a number of curious omissions. Not only does Kahan omit the entire experience related to the 1963 Limited Test Ban Treaty from the historical review—an experience which greatly influenced U.S. arms control thinking—but also he simply pays to little attention to the development of Soviet strategic forces and to Soviet attitudes toward the utility of these forces and to negotiated arms control. The foreign policy objectives met by Soviet nuclear weapons deployments are not directly addressed, and the problem of determining Soviet strategic intentions is not raised. Third, Kahan makes too little effort to distinguish between declaratory policy and force posture. The fact that the United States has unceasingly sought to enhance the accuracy of its ballistic missiles in the service of improved counterforce capability even while the official government policy rested on concepts of assured destruction and countervalue strikes, for example, cannot be appreciated from Kahan's analysis.

Finally, much of the second part of the book has simply been overtaken by events. With the unrelenting buildup in Soviet forces, the central problem in strategic analysis is clearly becoming the establishment of American criteria of strategic stability with an opponent that apparently has no interest in stability. Anti-satellite weaponry that pose potential threats to communication and early warning systems, the broad-based Soviet civil defense effort, and the prospective deployment of "gray area" systems—particularly cruise missiles—that can carry nuclear or conventional warheads and that can travel intercontinental ranges or be used in theater operations are some of the developments which are likely to have a major impact on future Soviet-American strategic relations. Unfortunately, they are not to be found in the author's discussion of future strategic issues.

In sum, there is much in this work to recommend it. It is a useful guide to what was and to what might have been. But unfortunately, despite the author's aspirations, it is ill-suited as a guide for what will be.

MICHAEL NACHT

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India's Nuclear Option: Atomic Diplomacy and Decision Making. By Ashok Kapur. (New York: Praeger Publishers, 1976. Pp. xiii + 295. $18.50.)

India's Nuclear Option was originally written as a Ph.D. dissertation at Carleton University. As such it is based largely upon published materials, supplemented by confidential interviews with Indian decision makers. Its primary focus is upon nuclear policy within the context of India's foreign and security policies.

India is a *weak* actor in terms of international security relations. As a consequence, security is probably the most important framework for the study and practice of India's foreign policies. Kapur's principal attention is directed toward India's nuclear policy and arms control strategy from the 1950s to the 1970s within this context.

Indian nonalignment is shown by the author to be a strategy distinct from alliance politics. Omitting an analysis of alliance per se, Kapur challenges a number of misconceptions concerning Indian nonalignment. India is seen not as a middle power, but as a power in the middle which is forcing stronger actors to interact with it while seeking to alter their behavior and attitudes. Thus India is attempting, vis à vis its nuclear option, to bargain without the capability of necessarily inflicting military harm. Its explosion of a nuclear device has made dialogue with New Delhi on the part of the stronger actors unavoidable. The resulting participation allows India both to negotiate more favorably with friends and to inject noncoercive norms and world order concerns in disarmament discussions between adversaries. India hopes to become not an object of activity for the superpowers, but a source of influence upon them, as well as upon lesser powers.

Kapur traces the transition from Nehru's nonalignment of the 1950s (which is still part of India's policy of peaceful coexistence) through the 1974 explosion of a nuclear device. This includes a thorough analysis of arms control and disarmament, India's nonalignment, Indian thought on nuclear politics, bureaucratic, personal and societal sources of her nuclear policy and the final decision to explode a nuclear device. The author believes that India's detonation will not necessarily hasten nuclear weapons proliferation, which is dependent upon a host of geopolitical factors varying with each potential nuclear power. He advocates changes in the Non-Proliferation Treaty, including allowing states to build a nuclear option where intention to embark upon a nuclear weapons program is absent.

India's Nuclear Option stresses the need for a conceptual elaboration which accounts for strategies emphasizing development of a nuclear option while foregoing conversion of the option into nuclear weapons. India has taken the first option and appears to have no intention of taking the second. Hopefully it may serve as a catalyst toward renewed efforts for the elimination of nuclear weapons and avoidance of the weapons option by other states. For all of us who are interested in the problem of nuclear proliferation, Kapur's detailed and valuable study of India as the latest nuclear power is highly recommended reading.

JOHN A. VOSBURGH

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Germany and the Politics of Nuclear Weapons. By Catherine McArdle Kelleher. (New York and London: Columbia University Press, 1975. Pp. xiv + 372. $15.00.)

Students of postwar West German foreign policy will welcome this book, which expands our understanding of what the fuss of the FRG's nuclear policy was all about. Covering the years from 1954, when the FRG waived its putative right to manufacture or possess nuclear weapons, until 1966, by which time the idea of a multilateral nuclear force (MLF) was dead, Kelleher has traced the elements of stability within changing contexts.

The cornerstone of German foreign policy was the second of three tasks enunciated in a 1955 government declaration—"the reunification of Germany, the problem of security, and European unification," which "presuppose[d] one another mutually." For Germans, who until at least the mid-1960s perceived the Soviet threat as real and continuing, there was only one guarantee of their security. Without a credible American commitment to defend Europe and the FRG, manifest in the presence of U.S. contingents and its deterrent threat, German reunification could never be achieved nor would West European integration have any meaning.

This central fact dominated FRG strategic policy throughout the 1954–66 period (and even now, as Kelleher shows in a final chapter updating the study). Having signed away at the outset its possibility of nuclear independence, and not having begun building a military establishment until 1956, the FRG was not in a position to force upon its allies its own strategic conception. Indeed, it had no peculiarly German conception to push! Reliance for security upon the United States meant that the FRG was reactive, not initiatory, on strategic issues.

Adenauer and his defense minister, Franz Josef Strauss, nonetheless tried to get maximum mileage out of the FRG's position. First, even the 1954 pledge, it turned out, was interpretable in *rebus sic stantibus* terms, and contained possibilities for greater West German latitude than had been originally assumed. It was hoped that raising such points aimed at forcing the Allies to end their discrimination against the FRG, would secure for it an equal voice in Allied decision-making circles and ultimately status as *primus inter secundes* in America's European alliance system. To the extent that the tactic reawakened wartime hostilities or concern about Tacitus' *furor teutonicus,* of course, it was counterproductive. Second, his American connection was instrumental in Adenauer's battles against both opposition Social Democrats and foes within his own party.

The Kennedy years upset all that. For one thing, the new president was determined to end the "German veto" on America's European policy that, he claimed, Dulles had permitted. Kennedy himself seemed to enjoy close relations with the more progressive SPD and especially Willy Brandt, Adenauer's opponent in the 1961 election. For another thing, de Gaulle's new line of independence struck a responsive chord in Germany—although Adenauer was determined to avoid having to choose between France and America. Finally, strategists in the United States were seriously questioning the assumptions and policies that had prevailed during the Eisenhower era.

Coming at a time when Adenauer was losing his own grip on power (and Strauss was sent into temporary political exile), such changes produced a crisis of mutual confidence. Kennedy charged the Germans with inflexibility. Adenauer, angered as he watched Kennedy destroy the special relationship that he had enjoyed with America through Dulles, was also concerned that the young president, in seeking Soviet-American détente, would bargain away the FRG's counters. Kennedy's spokespersons, thinking that they could "educate" their European allies about the need to substitute a flexible response for massive retaliation, were surprised when their arguments about cost-effectiveness and other supposed benefits of the new doctrine fell upon deaf or uncomprehending ears and were publicly (and apparently deliberately) misinterpreted.

What had happened? Dependence on the American guarantee as a sine qua non of their security had led Germans "almost inevitably to a status quo stance and to a hypersensitivity and overreaction to any perceived shift in American preferences or policies" (p. 6). Insufficient public or private discussion of a serious nature had left them woefully unsophisticated on strategic matters. Their questions, when Kennedy's representatives applied the hard sell, were: Does the new policy provide the security we want? How can we use it to secure other goals we consider important? Even Adenauer, once convinced that the ill-fated MLF represented a firm, if new, American commitment to the defense of Germany, became its champion, thus paving the way for a new German disappointment.

The failure to view strategic policy in other than an instrumental manner, often for cynical manipulation of domestic politics, had in effect rendered nescient precisely those who should have been at work developing a sound German policy. A blind pursuit of slogans and foreign policy goals that were logically inconsistent (although many Germans then would have been able at the drop of a hat to show their consistency in "real" politics) ultimately paralyzed Erhard's short-lived government (1963–66). And not until the SPD got into power could movement take place.

Kelleher's chronicle of these dozen years constantly interests and challenges us. For readers of this *Review,* however, unfulfilled expectations are also likely to crop up. To be sure, Kelleher meets one of her three goals: showing "the significance of Bonn's American connection within and beyond NATO for nuclear decision-making" (p. 6). But she does not meet the other two. As a case study of German foreign *policy,* first, it has a curiously unfinished aspect. The intensive analysis's terminal date of 1966 leaves us *in medias res.* We know that MLF is dead and that a Nuclear Planning Group has been created in NATO to provide a measure of joint control, but Germany's real role was far from settled in 1966–as the brief synopsis in the postscript chapter makes clear. (The study was doubtless more timely as a status report in 1967, when an earlier version was submitted as a doctoral dissertation.) As a case study of the West German policy-making *process,* we find too little attention paid to internal dynamics. Second, her brief argument to the contrary notwithstanding, Kelleher's book is not a case study of foreign policy making on the part of second-tier states, or at least one productive of anything more than commonsense propositions which a similar study on, say, France could refute in significant respects.

Other readers may lament lost opportunities. The volume's origins in interviews with West German leaders provided an unrealized chance à la Lerner and Edinger to get at the sources and function of their perspectives. Similarly, her brief discussion in the final chapter of the FRG's "options" makes us long for a more trenchant strategic analysis in the tradition of Buchan or Windsor.

If we concentrate on what the book is, rather than what it failed or did not intend to be, then we cannot fail to be impressed by Kelleher's grasp of a vastly complicated situation, her skill at clarifying it for the reader, and the contribution made through her judicious use of interview material.

RICHARD L. MERRITT

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International Agencies: The Emerging Framework of Interdependence. By Evan Luard. (Dobbs Ferry, N.Y.: Oceana Publications, and London: Macmillan, 1977. Published for the Royal Institute of International Affairs. Pp. xii + 338. $35.00.)

This is a useful book. The author sets as his objectives to help the lay person or the more serious student of international affairs through the maze of activities in the economic, social, communications and other non-security fields undertaken by the various agencies associated with the United Nations, and, in addition, to give some consideration to the structural problems of these agencies. In terms of these objectives, there can be no doubt that Luard has succeeded.

The bulk of the book is arranged in a series of 15 chapters bearing the titles of task or problem areas, from postal services to money and development. With respect to each of these topics, the author discusses (1) the nature of the problems and the international action taken to deal with them, (2) the organizational structures as they relate to the functions to be performed, and (3) the characteristic methods adopted by the agencies concerned to secure compliance by states with approved rules or attainment of espoused goals. A work of this kind that sets out to summarize the facts about a large number of complex institutions and activities is bound to be of uneven appeal. The chapter on money deals competently in 20 pages with the gold standard, the economic crisis of the 1930s, the Bretton Woods system and the causes and consequences of floating exchange rates; but, of course, more specialized readings are available to students about these things. More valuable perhaps to the general reader are the treatments of some lesser-known areas of activity. The chapter on sea transport is particularly well written; and a five-page account of the arrangements for the Antarctic provides a handy reference.

The framework chosen raises, by implication, some basic problems of approach. Should the book be organized by functional sector or by institution? The chapter titles suggest the former, while the chapter content leans towards the latter. There is a hard-headed practicality to this conceptually untidy solution: a problem is a problem when it becomes a matter for collective action.

Nevertheless, perception of problems has a way of getting ahead of existing institutions and programs, as the author recognizes in a reference to "institutional inertia" (p. 314). One might have envisaged a book constructed around a set of global issues defined more by perceptions of need than by institutional action. These might include some that have been the subject of major international conferences in recent years, e.g., the environment or food, and others that seem to have been particularly hard to deal with through existing institutions, like investment. These particular topics get somewhat short shrift compared with those handled by more fully developed institutions, such as telecommunications or civil aviation. An illustration of this same institutional bias is in the chapter on energy: rather than launch into an analysis of international energy politics, the author gives main emphasis to an account of the International Atomic Energy Agency, which, though interesting, is peripheral to the theme evoked by the chapter title. The chapter on social policy is an account of the bits and pieces of activities clustering under the rubric of social affairs in the United Nations, though the title might suggest a focus on such major issues as poverty, unemployment, discrimination or malnutrition and what is being done or not done about them. If, however, these emphases in the treatment of topics may be thought weaknesses in the book, they must be recognized as but the weaknesses of the existing structure of international action which the author rather faithfully presents *wie es eigentlich gewesen ist.*

The major drawback of such a useful general account is that its facts will become dated. Indeed, some important omission can be noted even at time of publication. The severely critical stance taken by the United States with regard both to UNESCO and ILO is not mentioned; and it is strange to read a book dealing with international economic policy published in 1977 that does not mention the agenda for the New International Economic Order.

There is a conservative bias to the author's approach that colors his reformist intent. The basic goals and methods of the various agencies are accepted as given. Indeed, they are presented usually as logically flowing from the objective characteristics of the problems discussed. The ways in which the selection and definition of goals and methods in practice reflects the configuration of world power is not considered. To have done so would have been to write yet a different book, with power rather than function as its organizing principle, and one that might have been more fundamentally critical of established institutions. Why, for instance, is there no good general Marxian critique of international organization? Yet without criticizing any fundamentals, Evan Luard does include a number of judicious reflections upon the shortcomings of some agencies and programs: the tendency of the IMF to force rich-country desiderate upon poor-country borrowers; excessive bureaucracy in FAO and authoritarianism generating low staff morale in UNESCO; the inability of talk to build houses through the UN's social activities; and reservations about the sanctity of tripartism in the ILO. His chapter on coordination and the problems of planning and fixing priorities in and among international agencies shows a keen sense of the bureaucratic politics involved.

The academic reader looking for a new contribution to the theory of international organization will be disappointed by this book. Those whose habit it is to look at the first and last chapters, skipping the middle, may come away with a poorer impression of this book than it merits. Its worth is in the solid middle. The start and finish do little more than restate David Mitrany's functionalism, adding the qualification that politics do intrude upon technical work and that something more than just functionalism is needed, namely "a deliberate political act of will by member governments" (p. 327). Coming on the next but last page of the book, this reads as a pious hope.

The author clearly recognizes that the authority of international institutions (they have, he cogently points out, "considerable *authority,* but no power," p. 288), and the techniques that they have been most successful in using to secure state compliance, rest upon consensus. His main proposals for strengthening international action for the future assume implicitly the existence of consensus, e.g., when he envisages a clearer definition of global priorities, and a concentration of decision making in smaller executive bodies. At present, however, signs of crisis and impasse in a number of international institutions seem to indicate that consensus based upon a configuration of world power prevailing in the post-World War II period is eroding, without any new consensus yet taking its place. The abiding weakness of international organization scholarship—and not just of this book—is that it has rarely come to grips with the real political parameters of international institutions.

ROBERT W. COX

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Uncertain Greatness: Henry Kissinger and American Foreign Policy. By Roger Morris. (New York: Harper and Row, 1977. Pp. vii + 315. $10.95.)

There is a bit that is useful, but much more that is not, in this disordered tracing and rambling critique of Henry Kissinger's eight years of almost single-handed direction of American foreign policy. What strength the book has derives largely from the author's eyewitness account of the evolution of American policy and the workings of the policy process from his vantage point as a National Security Council staff member, serving from the start of the Nixon administration until his resignation in May, 1970, over the Cambodian invasion.

The focus is on the role of personality and personal relations in making foreign policy. American foreign policy is portrayed as the outcome of the struggle between Henry Kissinger, in ambiguous and tenuous alignment, with the president (first Nixon, then Ford) against the other major and minor foreign policy players in the Washington drama. These include cabinet officers, agency bureaucrats, ambitious staffers, members of Congress, and the press. Much of the early, and strongest, part of the book plots Kissinger's moves in reorganizing the National Security Council. This mechanism was the institutional instrument of the Kissinger-Nixon alliance for the gradual assumption of control of foreign policy. The NSC system centralized decision making in the president under the strong control of his chief national security advisor and legitimated the effacement of traditional actor influences (like the Pentagon). It succeeded more by insulating the president from his natural enemies than by dominating them. Here, energy, secrecy and dispatch counted much against these "foreign" powers composing the foreign policy establishment, both within and outside the formal structure of the government. Even before President Nixon took office, the plan for the NSC system had been skillfully slipped past William Rogers and Melvin Laird, designated heads, respectively, of State and Defense. Neither they, nor their advisors, sensed that the new procedures would narrow their access to the president and decrease their influence over presidential decisions or unexpected initiatives, like the opening to China. The "*coup d'état* at the Hotel Pierre," the title of chapter 2, was hatched *before* the other major actors were in place. Kissinger's role in neutralizing the president's competitors and his mastery of foreign policy were indispensable. Laird was undercut either by playing on Nixon's fear of his presidential ambitions or in striking a tacit alliance with the Joint Chiefs on bombing North Vietnam in exchange for flexibility on SALT. Rogers' inexperience, lassitude, and ineptness was no match for Kissinger's alliances with Rogers' bureaucratic and politically appointed subordinates, like Elliott Richardson. Morris' glimpses into the inner workings of the NSC on the Nigerian Civil War or the Pakistani-Indian War, areas where Morris appears to have first-hand knowledge, provide a revealing picture of the personal and idiosyncratic quirks of the policy process (e.g., Nixon's tilt towards Pakistan).

The book is useful, also, for its catalogue of the clear failures and the doubtful successes of the Kissinger record in foreign policy: the shock to Japan in the move towards China and protectionist monetary and commercial measures; indifference to Pakistani suppression of Bangladesh and the un-*realpolitik* tilt to support a tattered and tottering regime rejected at home and defeated abroad; the fumblings over Cyprus after the courtship with the Athens generals; the covert attack on the Allende regime in Chile; the belated discovery of Africa; the costly and lamentable extension and duration of the Vietnam War for no useful purpose or appreciable strategic gain; the morbid, pernicious preoccupation with plugging internal leaks that contributed to a mentality disposed to illegal wiretaps and break-ins; the failure to reform the foreign policy bureaucracy; the refusal to forge a partnership with Congress in foreign policy; the willing seduction of the press; drift in Europe; hesitation and confusion in addressing the world economic crisis and the North-South confrontation. On the other hand, Kissinger and Nixon receive high marks for China and détente with the Soviet Union, which, in Morris' words, "helped the world come to terms with the Russian and Chinese revolutions" (p. 298). Credit is also extended for SALT and Kissinger's shuttle diplomacy in the Middle East. Also of interest is Morris' view that Nixon's role in these events, especially in the opening to China and in the peaceful engagement of the Soviet Union, deserves greater weight than conventional wisdom is ready to concede.

What is disappointing is the insular perspective from which the book is written. The world is made to revolve around the Washington merry-go-round. Nations, peoples, regimes, foreign leaders and world economic forces are still viewed either as functions of American writ or as susceptible to its physical power and moral claims. Kissinger and Nixon are criticized for having failed to intervene enough (e.g., in favor of the Ibos in Nigeria) or not wisely (e.g., in Vietnam and Chile). Morris never examines seriously the assumption on which the Nixon-Kissinger diplomacy was based: "that America lacks the power to control much of what happens in the world" (p. 297). Much of his criticism of Nixon and Kissinger's decisions and actions rests on the questionable assumption of an omnipotent America whose effective power lies in its example and moral superiority, not its wealth or military prowess. Beyond American shores, not in Washington, lies the intractable world with which American foreign policy must deal with means, alas, unequal to the tasks and responsibilities at hand. It is a world uncongenial to American institutions and democratic ideals. It is a world of threats to world order from internal fragmentation, national strife, or global revolutionary forces, like the Soviet Union; of chronic economic pertubation and dislocation in monetary and commercial relations; and of potential strangulation or blackmail of the West arising from the control of needed raw materials by hostile powers.

The issue is not whether American foreign policy should be based on moral principles. Who would say nay? Morris' shallow, if well-intentioned, appeals to high purpose provide little help in deciding between the conflicting choices presented by a world divided against itself. As Kissinger understood and Nixon sensed, the preservation of American democratic institutions, security, and solvency depend partly, but critically, on turning that division to American power and purpose through conflict management where American power, however limited, can still have weight and salutary influence (e.g., the Middle East) *or* managed conflict (China pitted subtly against the Soviet Union) where the protection and prosperity of American life lies in the divisions of one's adversaries. Morris is astonishingly blind to those obvious strategic principles informing the moves and maneuvers of the Nixon-Kissinger diplomacy. Equally puzzling is his pitiable plea in the final pages of 300 pages of unbridled criticism of Kissinger's personal diplomacy that Kissinger should personally intervene again as critic and gadfly in the interest of reforming and humanizing American foreign policy. That's a long walk for a short drink of water.

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Modernization and the Transformation of International Relations. By Edward L. Morse. (New York: Free Press, 1976. Pp. 203. $10.95.)

This "extended essay" poses the question: what are the effects of modernization on the structure of international relations? It defines modernization as the process of emergence of "highly modernized" societies, that is, societies characterized by a high ratio of consumption of inanimate energy. It uses this term, in effect, as a shorthand for the economic and social transformations of the twentieth century, those transformations which have produced a "social order" that is "qualitatively different from anything that preceded it in human history." It distinguishes modernization from the scientific revolution of the seventeenth century as well as from the rise of the nation-state.

The effects of modernization are seen as complex and sometimes contradictory but they are said to include: high interdependence, the development of a global system, growth of transnational activities and a general effect that makes the contemporary international system "transitional." In turn, these processes are perceived to have wrought significant changes in the "classical" system of "Westphalia" that found definition in the seventeenth century, such that present-day international society no longer fully reflects the assumptions of that system. The transformations are attributable to long-standing ideological challenges which both liberalism and Marxism presented ever since the onset of liberalization; also to changes in the foreign policy process of modernized states, the growth of several forms of interdependence, and the modification of international values (as seen in higher standards for the use of force).

The author is the first to admit that his analysis does not represent "a well-substantiated theoretical framework" or an "entirely convincing linkage between the process of modernization and changes in statecraft" (p. xvii) and I am not inclined to disagree with him on that point. The overriding impression one receives upon reading this essay is its inconclusiveness. The subject is, of course, vast and difficult and yet, despite the large number of topics broached and the numerous authorities cited, the connections between modernization and world politics remain elusive and at most, tenuous, and easily subject to contrary interpretations. Whereas the author himself opens his essay with the confident assertion that "modernization represents nothing less than the complete transformation of international relations" (p. 1), by the time he has reached the concluding chapter he is less sure of himself: "at the same time . . . it is plausible to assert that the Westphalia system remains dominant" (p. 178).

On the whole, one would not want to disagree with the author's basic feeling that the classical system of international relations needs substantial overhaul and, if you like, modernization. Nor would one want to quarrel with his conception of the several streams along which modernization is making an impact on that system. But the question whether world politics has now attained a "modern" form remains unsettled and in contention. And the author faces an added problem because of the terms he is using. Historians are generally agreed that the "modern" period, and the modern system of states, begin at about 1494. How then, do you "modernize" a "modern" system? He sidesteps the problem by calling it "classical" but he also generally neglects to draw on the writings of historians.

The argument is inconclusive in part because the principal question about the effects of modernization is artificially isolated from the rest of knowledge about the evolution of global structures. The equally plausible and possibly more interesting question, of the effect of world politics upon the process of modernization is completely ignored.

The way in which the "classical system" is identified with "Westphalia" is not helpful either. For even if the concept of "Westphalia" is significant in the story of the international law in the European arena, and evocative of the consolidation of sovereignty and the abandonment of notions of supreme authority, it does little to illuminate the dynamics of global politics in modern history and especially the role within it of certain powers that performed global functions. Given an inadequate conception of the "classical" system, it is difficult to say what, precisely, it is that is being transformed through modernization.

A definitive answer to the questions raised in this essay calls for more than a survey of ideas and ideologies. Even at its best such a review of the literature can be no more than a preliminary clearing of the underbrush. For the construction of a new theoretical edifice much more is needed: most of all, a good grasp of macrohistory.

GEORGE MODELSKI

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The Game of Disarmament: How the United States and Russia Run the Arms Race. By Alva Myrdal. (New York: Pantheon Books, 1976. Pp. xxvi + 397. $15.00.)

This work, written by a long-time participant in international disarmament efforts, is addressed primarily to decision makers, but also to experts and concerned citizens. Readers may find the book valuable for its broad survey of arms control initiatives and for its portrayal of the public disarmament efforts of the superpowers as a kind of game. Perhaps its greatest usefulness is in providing a "nonaligned" perspective different from that of most American writers who "are all addressing Washington" (p. xii). Americans have tended to view arms control measures as disinterested technical responses to a universal human problem; Alva Myrdal's approach is somewhat more political. It is refreshing to hear someone object to the SALT negotiations because they are limited to the two superpowers (p. 103), or to hear the hegemonic character of the Non-Proliferation Treaty frankly admitted (p. 168).

When we turn to Myrdal's evaluation of specific arms control proposals, we encounter some surprises. In spite of her passionate commitment to disarmament, Myrdal has a negative assessment of almost every arms control agreement reached in recent decades.

The "intent of the [Washington Naval] Treaty [of 1922] was *forfeited* by the qualitative improvement of cruisers and destroyers" (p. 67; italics mine). The Partial Test Ban Treaty of 1963 was a "rude awakening," since "the public was too easily satisfied with the ostrich-like solution of driving the tests underground—which had no effect on the major objective. . . ." The Non-Proliferation Treaty was "extremely unfair" and "falls short of all reasonable expectations as a disarmament measure" (pp. 170, 171).

Myrdal denounces the UN convention prohibiting bacteriological weapons because it does not provide for verification and because it did not include chemical weapons, thus slighting its effect in creating the climate for unilateral legal prohibitions against biological weapons by several major powers (pp. 268–77). Finally, the terms of the SALT ABM Treaty are scorned because "these . . . are not disarmament but non-armament measures" and because the permission to have two ABM complexes "was not restriction but quantitative expansion" (pp. 104–05). While it is true that none of these agreements was all that it claimed to be, Myrdal's approach to disarmament can be described as *all or nothing.*

This approach risks overlooking useful arms control measures that are practicable. After all, the Washington naval treaty, for example, did suspend two competitive drives for naval supremacy, markedly decreased the tensions between three major powers, and saved them vast amounts of money. On the other hand, the "all or nothing" approach may produce disarmament measures that would actually be dangerous to peace. Suppose that the arms race had been "stopped" by treaty in the mid-fifties. Strategic forces were then so fragile that they had to be used in a crisis or sacrificed, creating a powerful motive to strike first. It was the continuation of "the arms race" that provided missile silos and ballistic missile submarines, making war less likely through mutual fear rather than more likely. At about the same time, the application of the new military doctrine of deterrence—that what is vital is the capacity to retaliate after a first strike—make these new technological possibilities usable in maintaining peace. (If one considers that the most important specific aim of arms control is to reduce the probability of war, the development of deterrent theory might be the most effective arms control action of the century.)

Myrdal is not aware of these complications, which causes some confusion in her argument. She is a partisan of a purely deterrent strategy in its most extreme form (Minimum Deterrence) and would rely primarily on ballistic missile submarines, but she also complains of the "nonstop arms race going on in the underwater environment" (p. 101) that made her own position possible. Similarly, Myrdal says that "spy satellites" are best for verifying the ban on missile tests she seeks (p. 133), while uneasily admitting that these satellites are "by-products" of the arms race" (p. 295). If Myrdal reflected further on these tensions within her argument, she might be compelled to ask whether general opposition to the quantity of arms or the point in technological history at which they appeared (the "qualitative arms race") may not be less fruitful than case-by-case examination of the effects of specific weapons on specific arms control goals.

The SALT T agreements may have produced, through bargaining chips and bureaucratic payoffs, more weapons rather than less. In the aftermath of this experience, many American arms control writers have begun to grapple with the questions whether there may not be a self-defeating element in international negotiations and pacts as a means to arms control. Mrs. Myrdal's disappointment with past arms control agreements might have led her to a radical questioning of earlier approaches. Her proposals (pp. 332–31, for example) represent instead an intensification of traditional methods.

Myrdal's book is thus passionate and learned, but not thoughtful, as many further contradictions also indicate. She argues, for example, that there is no need for tactical nuclear weapons to defend Europe, since the United States "need only detach some of its [ballistic missile] submarines . . . ready to fire in case of an attack on Western Europe. That should be sufficient to deter any such attack" (p. 50). On the same page, however, Myrdal says that "the security originally pledged them [the European NATO partners] has necessarily *vanished* with the developments" (italics mine). In the case of Japan she similarly questions the American deterrent "umbrella," observing that "it is unlikely that the United States would accept the destruction of its own homeland to save Japan" (p. 57). The contradiction is inescapable. But the statement concerning Japan also raises a second problem. It is precisely the argument made by General Pierre Gallois to justify nuclear proliferation: if nuclear war means suicide, no country will fight such a war to defend another country, and the non-nuclear countries can only be secure by acquiring their own deterrents. But Myrdal is totally opposed to all nuclear proliferation. On this issue there arises a third contradiction. One of Myrdal's most important proposals is dismantling secrecy (pp. 302–16) including, eventually, secrecy about nuclear weapons technology (pp. 302, 315). Nothing, of course, would advance nuclear proliferation faster.

Myrdal relies throughout on pure deterrence—that is, "countercity" attacks (pp. 115–16)—while opposing all efforts to shift weapons to military targets or develop a capability for limited nuclear war (p. 130; cf. pp. 39–40). But she is uncertain that deterrence will work, since she seems to hold that the arms race creates a substantial risk of war (pp. 7, 17 ff., 130, 317). In the event of such a war, her preferred military doctrine will make civilians the sole targets. It comes as something of a surprise when Myrdal eloquently opposes the barbarity of area bombing (pp. 247–55). In a moment of forgetfulness, she even protests against the exclusion of nuclear weapons from proposed legal rules against attacks on civilians (p. 259). Thus amended, the rules would make deterrence unlawful.

These inconsistencies reveal an unwillingness to face the hard choices that our world, which is not of our own choosing, may require. Of course, her book is not the argument of a scholar writing for scholars but an activist's call for commitment and action. But when action occurs without reflection, it may defeat itself.

CHARLES H. FAIRBANKS, JR.

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In Search of Global Patterns. Edited by James N. Rosenau. (New York: Free Press, 1976. Pp. ix + 389. $15.00.)

James Rosenau has compiled an interesting collection of 40 primarily original articles written by a combination of young/old and quantitative/qualitative students of comparative foreigh policy who attempt to "portray failures—and to highlight the problems that must be solved if patterns are to be uncovered and knowledge cumulated" (p. 1). The result of three conferences convened by Rosenau, the papers focus on three distinct, though complementary, topics: (1) the origins and motivational dynamics of the search for global patterns; (2) a philosophical perspective on the reasons for the failures of the search and how they might be overcome; and (3) examples of how the search should proceed from this point.

The first section of the volume provides personal perspectives on the leading research projects in international politics. Even though all authors responded to the same questions concerning where they have been and why, the section has three distinct messages. First, the extremely interesting discussions of why each research project proceeded as it did should serve as a valuable introduction to the projects for future students of foreign policy. The most impressive facet of these articles (especially those by Rummel, Alker, North, and Guetzkow) is the degree to which the authors can be seen to be pursuing the same objective—a theory of comparative foreign policy. The second message is that even though tremendous effort has been expended, the projects have not successfully produced the desired theory, primarily because each started with little existing theory or data. Despite the "failure" to produce the desired theory, the project leaders do feel that the projects have made a contribution by providing a base on which others can build. Finally, several of the authors provide well-reasoned suggestions for future research strategies (see especially Alker, Choucri, and Holsti). The general prescription that emerges is that cumulative knowledge will result only to the extent that theoretical and empirical considerations can be coherently mixed.

The second section of the volume specifically addresses the following issues from a philosophical point of view: how successful has the search for global patterns been and how can progress be made toward the stated goal of cumulation? Not surprisingly, there is near unanimity that the search has not been entirely successful (Bounton, Singer, Rosecrance, Jervis, Hopmann). Because much work remains before knowledge of comparative foreign policy can cumulate, the authors of the second section suggest strategies for future work. The suggestions are wide-ranging and include: the adoption of a mathematical approach (Zinnes), the integration of existing theory (Rosecrance, Siverson), utilization of the scientific method (Singer), a focus on middle-range rather than general theory (Hopmann), and the restructuring of academic priorities (Jervis, Bremer). Despite being interesting, the suggestions are not as useful as they might be because of two problems. First, the impact of the arguments is undercut by the editor's failure to establish a working definition of "cumulation"; consequently it has been interpreted differently by different scholars. Because of this terminological problem, it is often difficult to compare the proposed solutions because they seem to be directed at totally different final products. Even without one definition, the suggestions would have been more useful if the authors had stated that their suggestions were conditional on a specified definition of cumulation. Second, little consideration is given either to the problem of how theoretical and empirical concerns can be properly merged into a coherent whole (as the authors of the first section wish to do) or to the limits of inductive inference for the cumulative enterprise. The major consequence of the failure to discuss (or even raise) the philosophical issues relating to the "coherent mix" is that the general prescription from section one is not addressed. Since the philosophical principles underlying such a mix are quite controversial, it would have been useful for this section to raise and investigate these key issues.

The task of the third section is to lead by example rather than by philosophical principle. Based on the articles in this section at least two approaches might be followed: (1) to develop a general theory of foreign policy or (2) to organize work under the rubric of an existing framework. Promoters of the first approach (McGowan, Gillespie, Thorson, and Harf) assert that the principal reason for the lack of cumulative knowledge is the absence of a general theory of foreign policy; that is, no existing theory has sufficient scope to identify and integrate the key components and relationships of comparative foreign policy. The authors offer various alternative general theories which could provide direction by postulating the necessary components and suggesting ways in which they can be integrated. The second approach is to use an existing framework. With the exception of the four previously cited articles, most of the articles remaining in section three look to Rosenau's pre-theory for direction. If these scholars are to be emulated, future research can focus on "solving puzzles" within the context of the framework.

Clearly the two approaches are at odds with one another; one is based on a typology, the other on theory. I would argue that neither is entirely sufficient. First, the framework approach strikes me as very narrow, being based on the widely criticized Rosenau pre-theory (e.g., McGowan, p. 227), because it explicitly excludes a substantial body of very interesting and important work, the cognitive process approach of Holsti and Jervis. Second, the general theory approach is too vague. Formal structures from other disciplines cannot be mechanically applied to the study of foreign policy unless explicit and plausible connections have been established between the formal theory and the study of foreign policy. The general theory approach cannot provide the needed direction until these connections have been made and have been shown to be both insightful and suggestive.

To succeed in his ambitious enterprise, Rosenau's collection would have had to resolve issues which have confronted all branches of science (e.g., Jervis). Needless to say, these issues remain. Nevertheless, the book is to be commended for having raised and tried to answer them. The lack of definitive answers is attributable to the fact that there is no easy or mechanical path to knowledge. What is required is insight, ingenuity, and persistence. This volume exhibits a measure of all these.

CHARLES W. OSTROM, JR.

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The Men Behind the Decisions: Cases in European Policy-Making. By Glenda Goldstone Rosenthal. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. 162. $13.00.)

Previous research and writing on the European Community (EC) has been largely theoretical in nature or else has consisted of individual case studies (sometimes informed by theory) or broad descriptive and historical treatments. This useful book is an original addition to our knowledge of EC decision-making processes. As such it goes well beyond previous studies, particularly in its comparative dimension.

In her effort to supplement the limited descriptive and theoretical materials on EC processes, Glenda Rosenthal examines five decisions as individual case studies: the decision to abolish restrictions on free movement of workers within the EC (July 1968), the conclusion of association agreements with Morocco and Tunisia (March 1969), the proposal for economic and monetary union (February 1971), the linkage of structural reforms in agriculture to price increases (March 1971), and introduction of generalized preferences on imports from developing countries (June 1971). The author has chosen the cases in order to cover substantial variations in time, subject matter, personalities, institutions and groups involved, and external pressures. She then analyzes each of these five cases from the perspective of three separate conceptual schemes or images of decision making.

Obviously it is impossible to assess the independent impact of six case variables (time, subject, personalities, etc.) filtered through three conceptual schemes using only five case studies. This underidentification problem affects most case study research. However, the author wisely chooses to stress the three conceptual schemes of decision making rather than to focus upon the case variables. She has formulated these conceptual schemes based on the major approaches adopted in earlier literature on decision making in general and the EC in particular. The first of these she terms "intergovernmental politics." This image perceives EC decisions as essentially a function of national political leaders acting through traditional diplomatic processes in terms of national interests (p. 3). Her second conceptual scheme, "Grass Roots, Interest Group and Parliamentary Pressures," is one which perceives decision making as the outcome of public pressures by the public or through interest groups and parliamentary representatives (pp. 4–5). The third approach, "Elite Networks" treats decision making as the result of action by a small, closely knit group or "old boy" network (pp. 5–6).

Rosenthal develops each case study perceptively based on her own interviewing and scrutiny of the relevant documentary materials. In her analyses she treats the three approaches to decision making not as individually sufficient or as mutually exclusive, but as sets of lenses which provide alternative perspectives and insights. Perhaps her most significant conclusion involves the importance of elite networks. For Rosenthal, "The most important link to emerge from the present research was the capacity of individuals and small groups to influence decisions" (p. 134). In addition, and in contradiction to the studies of Coombes and of Lindberg and Scheingold, she finds that decision-making analysis in the EC is best appreciated by understanding that there is not one process but many different processes of decision making in the Community, and that, further, there exist many different methods of influencing decisions.

Rosenthal's book is useful not only for the sophisticated analysis of EC decision making and of the rise of an elite decision-making network, but also because of the possibilities it conveys for treating the EC as a nascent political system in its own right. The book thus helps us to move beyond the familiar debates about the Community as narrow customs union versus emergent supranational body.

Two relatively minor problems characterize this book. First, the author is occasionally imprecise in her terminology, particularly in expressing precisely what she proposes to hypothesize. Perhaps the clearest statement of her intentions occurs (p. 14) when she writes, "The interviews . . . crystallized and confirmed an idea that had been in my mind during five years as a participant-observer: it is the men who govern the processes and not the processes that govern the men." Second, in defining the Intergovernmental Politics approach, she discusses the high politics/low politics distinction as one inherently of substance. Instead, this distinction could have been made more effectively on the basis of how an issue is treated, since many different issues can become subject to politicization. One additional caveat: although the author writes clearly she tends to lapse into the passive voice. (In fairness I must add that this sin characterizes much or even most contemporary social science writing.)

In sum, this is a thoughtful and worthwhile work. It is likely to provide an important reference point for future research on European Community decision making.

ROBERT J. LIEBER

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Red Star on the Nile: The Soviet-Egyptian Influence Relationship Since the June War. By Alvin Z. Rubinstein. (Princeton: Princeton University Press, 1977. Pp. 383. $25.00, cloth; $9.95, paper.)

The difficulties inherent in measuring one nation's influence over another have long presented problems to political scientists seeking to explain foreign and domestic policy behavior. In this study, Alvin Z. Rubinstein traces the relationship between the Soviet Union and Egypt in the 1967–1976 period in an effort to determine those factors contributing to Soviet influence in Egypt which can be meaningfully measured. Rubinstein begins his book with an analysis of some of the traditional means of intra-nation influence measurement, such as trade and economic aid data, military assistance, U.N. voting statistics, exchanges of official visits, and joint communiqués. Rather openly dissatisfied with certain techniques employed by the behaviorist school of political science in studying Soviet foreign policy in the Third World, Rubinstein asserts that, given the data likely to be available to researchers, joint communiqués, if carefully read, will provide the most meaningful clues as to the current state of relations between the Soviet Union and the state it is seeking to influence.

Interestingly enough, however, Rubinstein concedes that Sovietologists and Arabologists differ as to the "precise weight" to be assigned to the communiqués, with Sovietologists giving them considerably more weight than Arabologists. Being a Sovietologist, Rubinstein assigns a great deal of importance to the analysis of communiqués; indeed, this is the primary methodological technique employed in the book, and there is even an 11-page appendix listing the key elements in each communiqué issued from 1967 to 1975. Given the fact that the Egyptians (as the author himself admits) pay less attention to communiqués than the Russians do, and even pay little attention to formal treaties (witness the ouster of the Soviet advisors from Egypt in 1972, despite the Soviet-Egyptian Treaty of 1971), one wonders whether Rubinstein would have done better to investigate, in more detail, some of the events leading up to the joint communiqués, since a number of omissions in the thread of events he presents in the 1967–1976 period, together with a series of somewhat questionable assertions, detract from the value of the book not only as a study of influence measurement, but also as a case study of Soviet-Egyptian relations during the period.

After a brief introduction, Rubinstein begins his study with an examination of Soviet-Egyptian relations in the two years following the June 1967 war. Here he carefully analyzes the domestic pressures on Nasser as motivating factors in his foreign policy, pressures which led to Nasser's initiation of the "war of attrition" along the Suez Canal in the late spring of 1969—an action reinforced by Soviet supplies of weaponry. According to Rubinstein, the

Russians were willing to gamble in taking this action (and were not too concerned about a western reaction), because (p. 87) Washington was "eager for SALT" and the Chinese were still bogged down in a "raging cultural revolution." His hypotheses, however, are open to question. In the first place, the U.S. was not only *not* eager for a SALT agreement in the spring of 1969, it was actually dragging its feet on negotiations. Secondly, the Sino-Soviet border battles of March 1969 effectively terminated China's cultural revolution; it was officially ended by the Ninth Chinese Party Congress which met the following month. Thus, while Moscow, in reinforcing Nasser's war of attrition, seems to have discounted the American reaction, this would not appear to be for the reasons Rubinstein suggests, but rather because the Russians were not unwilling to test American resolve and heat up the situation in an effort to weaken the U.S. position in the region.

With the death of Nasser in September 1970, a new chapter of Soviet-Egyptian relations opened. The Russians hurried to reinforce their relations with the new Sadat regime as first Kosygin and then Podgorny visited Cairo. Relations were to sour, however, in May 1971 after Sadat survived an attempt to oust him by the reportedly pro-Moscow faction led by Aly Sabry (an event which precipitated the Soviet-Egyptian Treaty of May 1971 as the Soviets sought to solidify their ties to Egypt), and deteriorated further in July when Sadat publicly opposed the Russians during the abortive Communist-supported coup in the Sudan. Meanwhile, Sadat was seeking to obtain weaponry from the USSR to launch a war against Israel as his self-proclaimed year of decision (1971) drew to a close. By the fall of 1971, however, the Russians were already involved in supporting India as it prepared for war against Pakistan and were in no mood—at that time—to help their rather fickle Arab ally against Israel. As the Soviet reluctance to supply Egypt with the desired weaponry continued into 1972, and as a Soviet-American "detente" appeared to have been created by the Moscow summit of May 1972, Sadat decided to expel the Soviet military advisors from Egypt and reassert Egyptian control over Egyptian military bases. Rubinstein cites the reason for Russia's acquiescence in Sadat's moves as an effort to encourage detente (p. 201), but fails to cite any supporting evidence for this hypothesis. An alternative explanation, that the USSR had invested so much in Egypt that it was unwilling to risk losing everything by actively opposing the expulsion order, would appear much more reasonable.

Rubinstein's account of Soviet-Egyptian relations during the 1973 war offers a number of useful insights into Soviet behavior, including the observation that Soviet posturing about a cease-fire in the early stages of the war may have been a ploy aimed at preserving Syrian gains in the war while at the same time disregarding Egyptian interests Rubinstein also justifiably criticizes Kissinger's performance at his news conference of October 12, 1973 in which he cited Soviet press and Security Council behavior as evidence of the "still tolerable" behavior of the USSR during the war rather than citing the ongoing Soviet military airlift and sealift to Syria and Egypt which would have led to the opposite conclusion.

Once the war was over, Soviet-Egyptian relations again soured as Egypt turned to the U.S. for assistance. Rubinstein discusses in some detail the growing Soviet disenchantment with Sadat during this period, but his contention that the Russians have increasingly come to deemphasize the Geneva conference (p. 313) is somewhat doubtful, given the continued Soviet emphasis on the conference as a means of preventing any more U.S.-mediated "separate deals" between Israel and its Arab neighbors.

In the final section of the book, Rubinstein reaches a number of conclusions about influence—none of which will prove very surprising to students of Soviet policy in the Middle East. In the first place, he asserts that an extensive presence is no assurance of influence. Second, he invalidates the hypothesis held by some political scientists that in a two-state relationship, the state which receives the greater number of visits will exercise "asymmetrical influence" over the sending state. Finally, and perhaps more importantly, he concludes that the donor derives more benefit from "the broad consequences" of a recipient's policy than from the immediate results of any contribution by the donor.

All in all, Rubinstein offers several good insights on Soviet-Egyptian relations in the 1967–1976 period, but the reader is left with the impression that the phenomenon of influence measurement needs more academic exploration.

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Field Theory Evolving. By R. J. Rummel. (Beverly Hills, Calif.: Sage Publications, 1977. Pp. 533. $29.95.)

No one has ever been able to improve upon, or overcome, Quincy Wright's classic analogy of field theory as a cheese full of maggots (in James N. Rosenau, *International Politics and Foreign Policy* [New York: Free Press, 1969] p. 446). Rummel's work does nothing to ameliorate the unpleasantness of this analogy.

Field Theory Evolving makes an attempt to answer several past critiques, as well as to show how field theory has grown and changed through the years. However, a number of technical and substantive problems remain. Space precludes a discussion of technicalities, but the more serious errors would include ignoring mathematical implications, using regression analysis while denying the regression model, and confusing statistical with substantive significance. We could, perhaps, overlook the technical slips if Rummel's version of field theory were substantively meaningful. Despite obvious improvements and attempts to increase substantive relevance, field theory is fundamentally unrealistic. This lack of realism stems from (1) the basic operationalization, (2) the incorporation of time, (3) the exclusion of feedback, and (4) the exlcusion of potentially critical variables. I will discuss these in turn.

(1) Operationalization. Field theory is operationalized using factor analysis. This technique has a place in data reduction, but in the present instance it is used to define empirically attribute and behavior spaces. If factor analysis is the proper technique to define these spaces, why don't the resulting factors resemble theoretical dimensions laid out by Wright (op. cit.)? Why are many factors odd combinations of variables that no decision maker in his or her right mind would ever lump together?

Additionally, factor analysis is a linear model, as is the fundamental equation of field theory that links behavior space to attribute space. A quick glance through our scientific journals would reveal that very few relationships are linear, additive ones. Most of the important relationships in international relations are curvilinear and multiplicative. A theory that is based on linear, additive relationships simply cannot capture the complex social and political world in which we live.

(2) Time. Although "social-time" is included in the theory, it is incorporated as a vector in the space of interest. Even if such an inclusion does impart a certain "dynamic" element to the theory, social-time is not calendar-time. This lack of calendar-time leads to a loss of predictive capacity. Thus, although the theory might be able to predict certain events occurring when certain combinations of attributes are present, we have no idea when that combination might occur. Further, Rummel never seems to come to grips with a fundamental empirical observation: that attributes generally change at a much slower rate than behavior. Hence, attributes cannot possibly account for short-term fluctuations in national behavior.

(3) Feedback. Anyone observing international relations is bound to notice that a state's behavior affects its attributes. This effect is at least as strong as the effect of attributes on behavior. We can note that many of the rapid, large, and/or discontinuous changes in attributes result from behavior. For example, major shifts in population size, industrial production, square miles of territory, etc., are usually the result of fighting a war. Rummel himself admits that behavior can affect attributes (p. 206), but claims that to include feedback in the theory would be redundant! "One way causation from attributes to behavior *should* be sufficient to capture the behavioral variance associated with feedback" (p. 208, emphasis added). Since Rummel is "capturing" a compound effect (direct plus feedback-related), the direct relationship that is measured will be overestimated. Furthermore, such compounding distorts the logical sequence of cause and effect. Finally, even if we grant that all appropriate variance *should* be captured, what if it's not?

(4) Variables. Predictor variables are another major source of distortion in field theory. Although the factor analysis incorporates some 236 variables, they are all on the national attribute and dyadic level. Nowhere do we find system-level variables such as the distribution of power. More importantly, nowhere do we find sub-national (i.e., bureaucratic and organizational) or individual-level variables. Rummel recognizes the particular criticalness of the latter; he refers to his *The Dynamic Psychological Field* (Sage, 1975) in his introduction. However, field theory, as currently structured, could not easily be modified to include such crucial variables. What good is a theory based on mysterious "driving forces" that can statistically account for 80 percent of the variance in national behavior when it does not specify the causal mechanism? Wars quite literally begin in the human mind, and any theory that neglects individual-level variables is seriously lacking.

All in all, despite the noble attempt at "reconstructed logic," Rummel's *Field Theory Evolving* is an affront to philosophers of science, an outrage to statisticians, and will be irrelevant to nearly all international relations scholars and political leaders.

CYNTHIA A. CANNIZZO

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Dynamics of a Conflict: A Re-examination of the Arab-Israeli Conflict. Edited by Gabriel Sheffer. (Atlantic Highlands, N.J.: Humanities Press, 1975. Pp. xxxiii + 369. $12.50.)

The year Israel celebrated a quarter century of nationhood after rebirth in 1948, a group of scholars (primarily Israelis and American academics) gathered for a conference designed to stimulate systematic thinking on possible solutions for problems Israel has faced with its Arab neighbors throughout recent history. These meetings, sponsored by the Van Leer Jerusalem Foundation, had other objectives too, among them to encourage interdisciplinary theoretical and practical thinking on these complex sociopolitical phenomena, and consider the contributions of academic work to policy formation in conflicts (p. vii). This book contains the central product of the symposium: 16 papers, mostly essays addressing (some) aspects of (some) issues confronting (some of) the states involved. It is not balanced with Arab views, but are decidedly Israely in sympathy. The papers are arranged and assembled under five categories: Background to the Conflict (3 pieces); Arms Race Problems (2); Legal and Political Factors (6); Psychological Aspects (1); and Hindrances to Peace (4). Overall, this is a well-integrated collection with surprising continuity in presentation and argument from one article to the next, albeit with occasional lapses into repetition and therefore redundancies which the reader must endure. But the style and lucid writing in the vast majority of articles reduces annoying effects brought about by this bothersome feature.

Does the book have a theme? Beyond some basic agreements—that the Arab-Israeli dispute is serious, that peace is remote, that basic positions of states remain unchanged (no startling insights here), possible candidates for overarching themes are: (1) regarding international stability: the implications and consequences of defining it as no war-no peace in the Middle East; or (b) regarding Israel: Israel as a deviant case in international politics; or (c) the relevance of linkage politics: Israeli foreign policy and effects of external forces.

The stability idea is introduced by Richard Brody, in a brief background essay outlining four essential features that set parameters for a Middle East conflict system. These include: an enduring Arab-Israeli animosity, inter-Arab disunity, economic differential among Arab states, and superpower involvement. Brody suggests that future research tackle stability issues by arriving at "estimates of the causal role these various elements play in the present, and recent historic systems" (p. 21). This theme is carried on by Colin Gray who analyzes negative and positive effects of arms racing. Articles by Evron, Baer, Sheffer, Horowitz and Quester discuss stability issues directly, or as recast into symmetry/asymmetry perspectives with respect to military power, deterrence, superstate backing, regional economic competition, misperception and mirror images, and policy bargaining stances of states involved.

That Israel is a novelty in world affairs is the theme developed by another background essay by Segre and Ben-Yaakov. By standard geographic analysis and assessments of sheer military strength, a developing society surrounded by enemy states and engaging frequently in battle with them has a remote chance for survival, and should in all probability be unable to achieve quick growth and thrive as a nation. Israel's position as extreme exception to expectations derived from conventional wisdom remains a curiosity. Papers commenting on this theme include those prepared by Quester, Arielli and Eban.

The linkage politics theme is eloquently stated by Abba Eban in the closing essay: "our aliya budget is determined not by ourselves but by the degree to which other governments open their gates or build up pressures congenial to departure. Our security budget is determined by fluctuations in the intensity of the hostility that surrounds us and by the momentum and dynamic of military technology. Thus the two major items in our national vote are dictated almost exclusively by extraneous influences" (p. 350). Most articles touch on the matter of external influences on Israeli policy, stressing outside control in conflict management. One might be left with the impression that Israelis do not feel there is any room for manipulation here, but their military performance at least, defies that conclusion. Two essays appearing back-to-back in the section, "Hindrances to Peace," offer divergent points of view concerning Israel's role in finding conflict solutions. Harkabi indicates that little can be done by the nation, as resolution depends on extra-Israeli components. Arielli, however, writes that Israel has changed its terms for a settlement and prefers continuation of a status quo to a settlement that would not conform to its territorial demands (p. 346).

What exactly is sorted out in this book? We are led to understand how a portion of the Israeli intellectual elite views its task in analyzing the conflict–their frame of mind in contemplating solutions, their sense of dealing with the issues as they arise and approaching the problems of the dispute package-style, where all must be solved more or less simultaneously. This prolongs a no-win situation for all the parties and perhaps defines present-day stabilities in Brody's terms. But the world is changing and almost everyone will agree that solutions are needed for the Arab-Israeli conflict. What is lacking in this compendium is a new or insightful look at unraveling the relevant issues or constructing scenarios that may be conducive to peace. A draft paper by Dror included with this collection, is the sole attempt at fresh thinking along these lines. Dror has provided the basis for a crude, but perhaps workable, conceptual model.

The general limitation of this work, contrary to promises made in the preface, that we learn little about future developments that may unfold in this volatile dispute, may be rooted in the descriptive mode of analysis used to explain the past. In fact, the entire book is basically history, not science. The popular and academic press has offered considerable effort at analyzing this conflict, ranging from polemics to scholarly treatment, coming from social scientists, diplomats, and affected citizens, aimed at understanding why the conflict persists, why it intensifies or what must be changed for solutions to be feasible. Do we require additional understanding of the past? What function is served by that? These scholars have done their jobs well–the articles make fine reading, with well-put arguments. However, a sense of urgency about the future really defines this conflict now, and a look into the future does not seem to be the preoccupation of these writers. Due to space considerations, discussions that followed paper presentations are unfortunately not included. Such ad hoc comments may have corrected the imbalance seen by this reviewer.

The symposium was held during 1973 before the October war, but articles were edited afterwards. Postscripts were appended to about half the pieces. Because Israeli performance differed so dramatically from the 1967 experience, it affected analysis. That is to say, generalizing on the basis of three major wars, at least for the current Middle East dispute, is insufficient. On the other hand, perhaps these articles were not attempts at generalizing but searches for a unique understanding of Israel's positions or some general truths as they follow from the Isreal case. The picture is more complex than it had appeared.

This book is unfinished in that it leaves the reader asking: well, what about future Middle East conflicts? What effects are brought about by changes in the situation such as UN resolutions on PLO legitimacy, a right-wing oriented government in Israel, Lebanese civil war, energy shortages and the disappearance of Kissinger from the foreign policy front? The generalist's orientation expressed by nearly all of the pieces in *Dynamics of a Conflict,* imposed serious limitations. One is anxious to know, and this occasion was the perfect format for asking, whether 25-year-old Israel will reach a golden anniversary and what will be the probable shape of things at that time. That information is not forthcoming.

KAREN ANN FESTE

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Storm over the Multinationals: The Real Issues. By Raymond Vernon. (Cambridge, Mass.: Harvard University Press, 1977. Pp. viii + 260. $12.50.)

In this volume, one of the most widely respected and prolific analysts of multinational corporations has set himself the task of presenting a grand overview of the nature and consequences of the multinational enterprise. The task is very difficult and Raymond Vernon only partially succeeds, in my view.

The book can be divided into two parts. The first part deals with the forces giving rise to the extraordinary growth of multinational enterprises, their basic characteristics, and various strategies guiding their behavior. The second part of the volume deals with the tensions these firms generate in advanced industrial states as well as in less developed countries and the prospects for managing the inherent contradictions between multinational enterprises engaged in transnational processes and states pursuing national goals.

Vernon associates the phenomenal growth of multinational enterprises and international economic interdependence with advances in communications, transportation, and computer technology. While these developments are certainly necessary conditions for the emergence of the modern multinational firm, political scientists are likely to question whether or not technological developments are themselves sufficient to explain the burgeoning of multinationals since World War II. A rich literature has emerged which associates the growth of multinationals and increased international economic interdependence with the existence of an international political/economic order under American hegemony. Vernon has little to say about how the international political and economic context within which multinationals operate shapes their growth.

In chapters 2–5 Vernon provides a lucid analysis of the basic characteristics and strategies of multinational enterprises. In this analysis he covers such familiar topics as vertical integration, diversification of product lines, geographical dispersion, ownership patterns, managerial patterns, and the struggle against entropy. The most impressive aspects of the analysis are its breadth and the disaggregation of behavior patterns which are too frequently dealt with superficially in other overviews of multinationals. Thus, in discussing internal decision making of multinational firms, Vernon draws distinctions among the typical functional divisions of a large enterprise (finance, marketing, production, government relations, labor relations) and indicates how relationships between parent and subsidiaries are likely to differ in the various functional areas. The introduction of even elemental distinctions of this sort is a welcome step beyond more conventional overviews. of multinational firms.

Political scientists are likely to be frustrated by the material in chapters 2–5, however. Vernon certainly appreciates the fact that differences among multinational firms' characteristics and strategies are of considerable importance to governments. On the other hand, he makes no effort to develop hypotheses about precisely *how* particular structural characteristics and strategies of firms are likely to affect home and host states' capacities to achieve various types of national objectives (economic, political, social). The volume would be of greater interest and use to political scientists if the material on the characteristics and strategies of multinational firms developed in the first part were more systematically linked to the material in the second part dealing with the tensions these firms generate in states at different stages of industrial development.

In chapters 6–9 Vernon examines a wide array of issues central to most overviews on the tensions created by multinational firms in and among home and host states—their impact on employment levels and structures, capital flows and foreign exchange rates, taxation, transfer pricing, and international and domestic competition. Throughout this analysis Vernon stresses the limits of plausible inferences from data presently available on these issues, rather than making bold assertions about the net impacts of multinationals on the political/economic/social systems of home and host states. I think his caution is well taken.

In his examination of multinational firms' impact on less developed states, Vernon covers terms of trade, overpricing of services, components, and technology in intercorporate transactions, and policies of host states. Yet, he basically feels that deeply ingrained sensitivities about exposure to international political/economic trends and dependence upon foreign firms and advanced industrial states are at the heart of tensions between multinationals and less developed countries. "The more explicit points of contention that the poor countries voice in their debates over the multinational enterprise, therefore, often represent nothing more than semantic hand-holds, tactical sallies in a struggle that arises from much more fundamental sources" (p. 148).

Throughout the book, Vernon sees multinational firms as being blamed wrongly for all variety of social/political/economic ills (hegemony, corruption, waste, income inequality, and pollution) which are present in all societies, including those which bar or carefully restrain multinationals. I find myself in substantial agreement with Vernon on this important point. Yet even if multinational firms cannot alone be blamed for these and other ills, we need careful analyses of exactly how each of these problems is exacerbated or ameliorated by various types of states. Vernon's book does not take us far in that direction—few works do.

Vernon concludes the volume with an overview of the prospects for managing relations between states and multi-national firms in the future. On balance, he concludes that multinational firms are likely to find themselves progressively on the defensive in their relations with states. Vernon fears that states' increasing effectiveness in harnessing multinationals to national goals in a beggar-thy-neighbor fashion will create a major source of international tension in the future. This is a particular threatening prospect in his view since he is not much impressed with the prospects for creating an international regime capable of effectively limiting national conflicts over investment policies.

In sum, this is a careful, informative, intelligent overview of multinationals and their relations with states. All students of multinational firms should examine it. It is not, however, an innovative book in terms of its presentation of concepts or data. In spite of the number of issues covered, the volume conveys little real sense of the interactive relationship between multinational firms and the international and domestic political/economic milieus within which they operate.

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The Troubled Détente. By Albert L. Weeks. (New York: New York University Press, 1976. Pp. xxiii + 190. $10.00.)

Unfortunately the title of Albert L. Weeks' new study is rather misleading, for, rather than focusing on the contemporary problems facing the Soviet-American relationship, he presents a broad survey of the history of "détente" that ranges from Lenin's policy of coexistence with the capitalist West to the present Soviet policy of competitive coexistence. However, in spite of the misleading title, *The Troubled Détente* does offer a concise and accurate summary of the history of Soviet relations with the United States and Western Europe.

Weeks begins the book with a nine-page glossary containing 33 terms whose meanings in Soviet usage often differ substantially from standard Western definitions. The general reader who is not conversant with the intricacies of Soviet political jargon will find especially useful the definitions of such concepts as, for example, "bourgeois pacifism," "correlation of forces," and "socialist democracy."

The main body of the text begins with a discussion of what Weeks calls "Brezhnev's ideology of détente," in which he places special emphasis on the importance in contemporary Soviet thought of recent changes in the correlation of forces—i.e., the relative strengthening of the political and military position of the Soviet Union and its supporters and allies vis-à-vis the West. In this section, as well as in later portions of the book, Weeks relies heavily on a relatively limited number of authoritative Soviet publications—e.g., *Marxism-Leninism on War and Army* published by the Main Political Administration of the Soviet Army and Navy, A. O. Chubaryan's *V. I. Lenin and the Formation of Soviet Foreign Policy,* and *International Communist, Workers', and National-Liberation Movement, School Textbook* published by the Higher Party School.

The second through fourth chapters of *The Troubled Détente* provide an overview of Soviet policy toward the capitalist world during the periods of leadership by Lenin, Stalin and Khrushchev respectively, while chapters 5 and 6 concern the development of the most recent period of improved Soviet relations with the West and the prospects for continued improvement of East-West relations.

On the whole, Weeks' sanguine evaluation of the future of détente, as well as his overall treatment of Soviet Western policy during the past six decades, is based on an informed understanding of both the theoretical underpinnings of Soviet foreign policy and the actual implementation of that policy. One can indeed agree with the claim made on the dust jacket of the book that *The Troubled Détente* is both timely and illuminating.

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The Politics of Crude Oil Pricing in the Middle East, 1970–1975: A Study in International Bargaining. By Richard Chadbourn Weisberg. (Berkeley, Calif.: Institute of International Studies, 1977. Pp. x + 170. $3.50, paper.)

Myth often shrouds discussions of oil policy because of the complexity of the commercial and political issues involved, the secrecy associated with the behavior of firms and governments, and the anxiety resulting from suspected manipulation of so essential a commodity as petroleum. Weisberg contributes importantly to the process of dispelling myth and uncertainty by carefully tracing the evolution of oil policy in the crucial years of the shift in control over price from the Texas Gulf Coast to the Persian Gulf. In particular, he provides additional evidence for three fundamental contentions of contemporary oil politics.

First, his work supports the thesis that in oil matters general equilibrium trade theory and much standard oligopolistic theory count for little. Instead, an approach is necessary which emphasizes relative power and bargaining ability among the three principal actors—the producers, the oil companies, and the consumers. Classical two-actor trade models distort reality wherein the key question remains who controls the market and why. Although it is still far from providing a systematic alternative model, political analysis continues to be central to the resolution of this question.

Second, the author demonstrates convincingly that the producer governments, not the companies, are currently in control of market relations, an issue which often is still debated and still misunderstood in the United States when visions of vertical divestiture arise as a technique for shearing off the majors from OPEC, allegedly thereby facilitating the collapse of the cartel. Third, Weisberg treats in balanced fashion the respective impact of the State Department and the major oil companies on producer government policy during the 1970–71 negotiations, concluding that at various times State made it "financially advantageous for [OPEC] not to take action, rather than to threaten or to use force" (p. 67). Confusion and inaction rather than conscious effort to oppose competition from Japan and Europe (a thesis proposed both in Britain and in a leading U.S. foreign policy publication) becomes a sufficient explanation for U.S. acceptance of price increases.

Not so satisfactory is Weisberg's failure to emphasize the significance of the fact that American crude oil production peaked in advance of the 1973 price revolution while U.S. demand continued to rise. The resulting oil import disparity undermined the influence which the Texas Railroad Commission traditionally exerted in price matters, allowing a shift of this power to the "swing producer," Saudi Arabia, and to the cartel. Likewise, one can question the author's assertions that political ideology has had little impact on the producers' pricing strategy. Although increase in per-barrel revenue was a basic objective of all producing governments, a pattern developed beginning with Gadafi's aggressive and untraditional threats of nationalization which placed Libya ahead of the conservative Gulf states in terms of demands. To be sure, they followed his lead, but as late as June of 1974 (pp. 86, 138) Saudi Arabia hesitated to raise prices as rapidly as had a number of the more radical states, or for such highly politicized purposes. It was Gadafi's ideological unpredictability and "imprudence" which initially broke the negotiations wide open.

Weisberg's analysis leaves unresolved the issue whether post-embargo Arab pressure on prices is motivated by political or economic objectives (or both), and if political, whether concessions in the Israeli-Arab dispute would (or could) result in oil price or production relief. Similarly, his view that the OPEC producers gave little attention to the threshold where energy substitution is likely to occur is not supported by published reports of advice given by OPEC consultants (see Marwan Iskandar, *The Arab Oil Question*, p. 73). Rather, the substitution threshold was so high and supply inelasticity so great that plenty of room existed for price increases in the short term without increases of production from new wells or alternate sources. Large-reserve states like Saudi Arabia recognized, however, that the longer-term consequences for them could be quite different.

One must strongly agree with the author's assessment of cartel government motivation, namely that the internal aspect of OPEC stability is largely a function of (1) the distribution of market shares, (2) revenue demands of the individual governments, and (3) their potential for increasing individual market shares. Drawn from secondary government and industry data sources, the study, in short, is a solid historical account of an important transition period for the international system.

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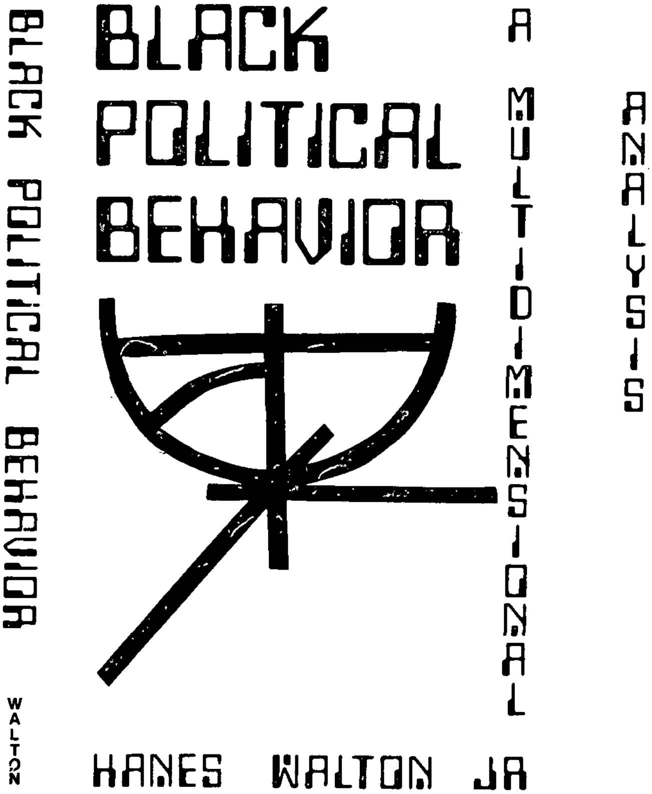
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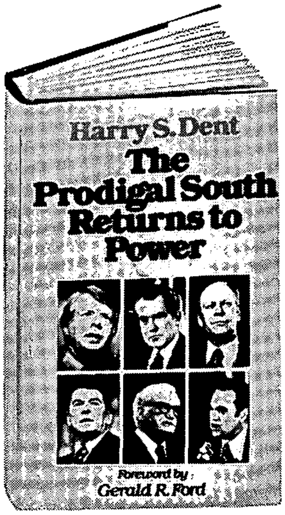

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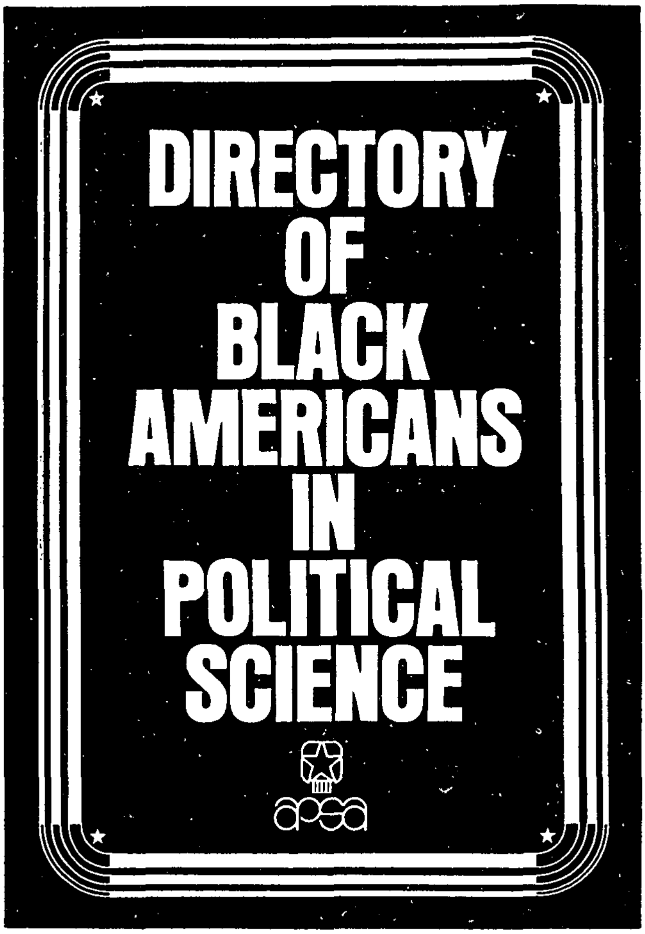

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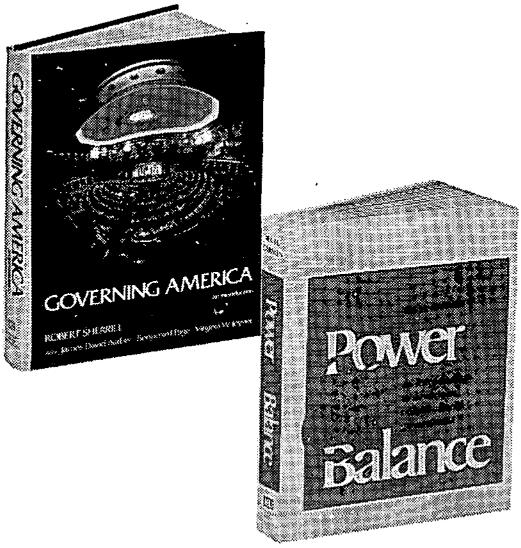

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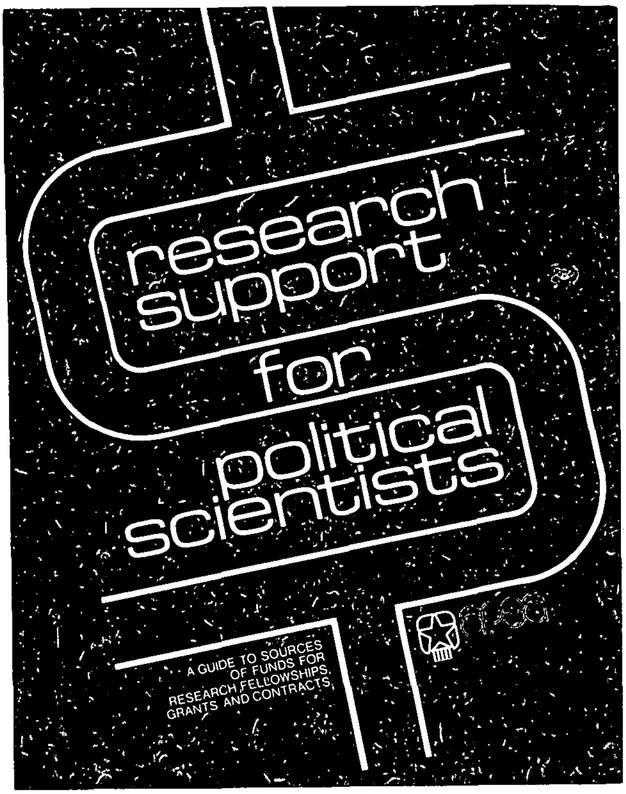
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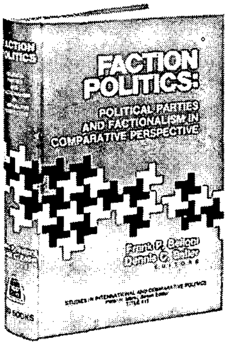
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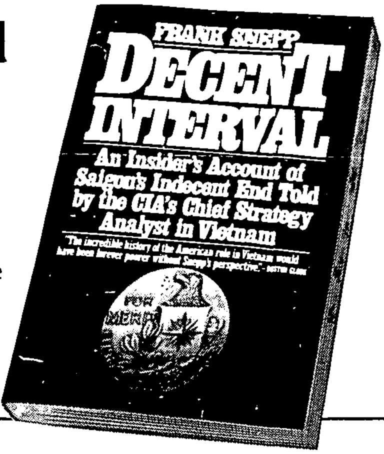
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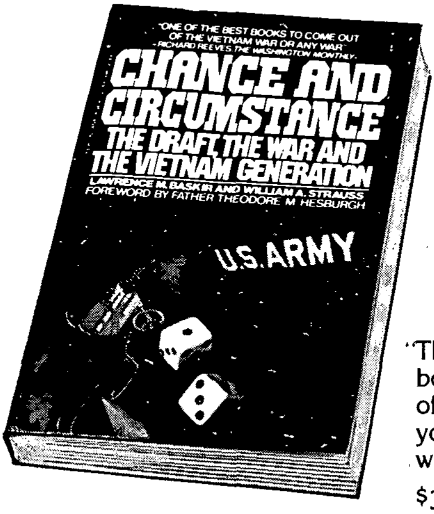
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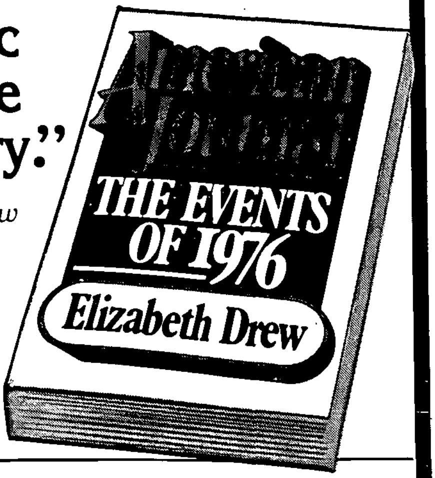
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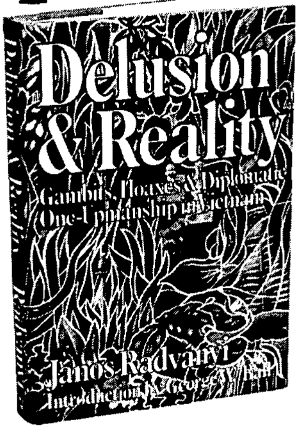
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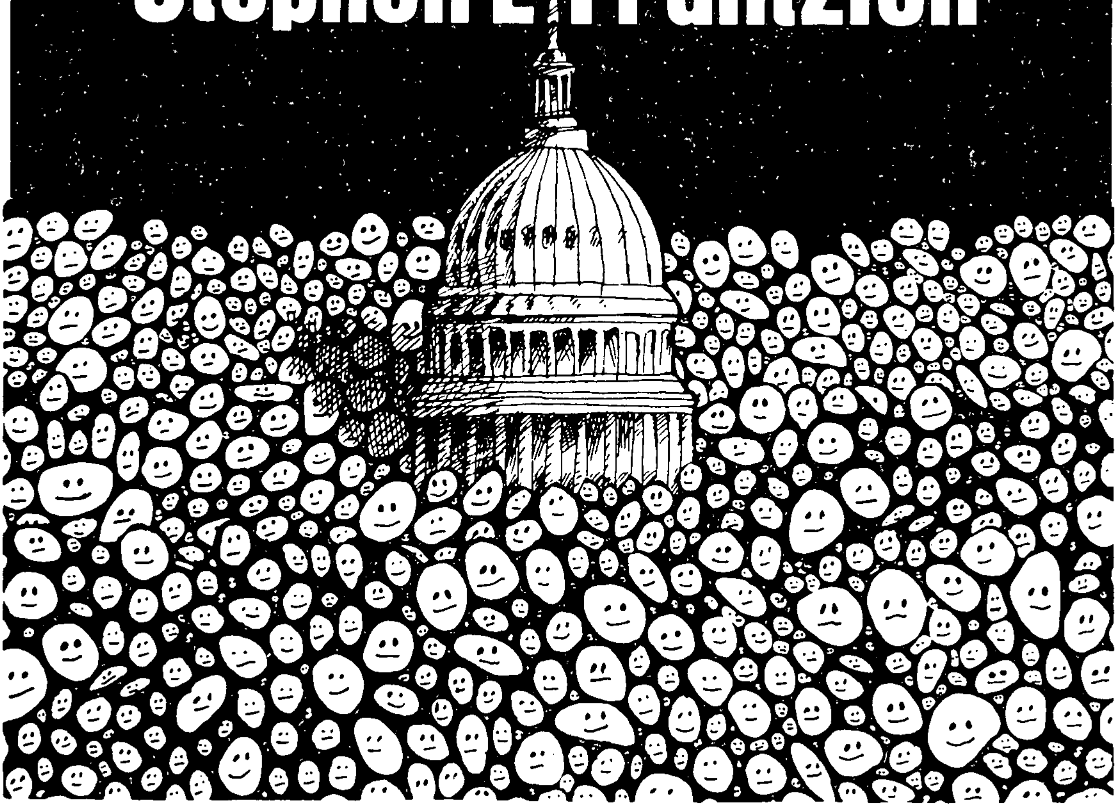

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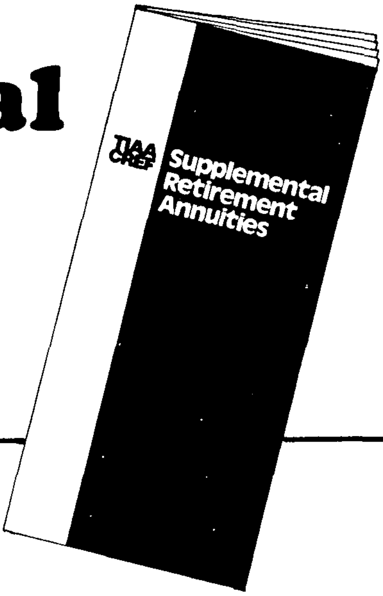

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